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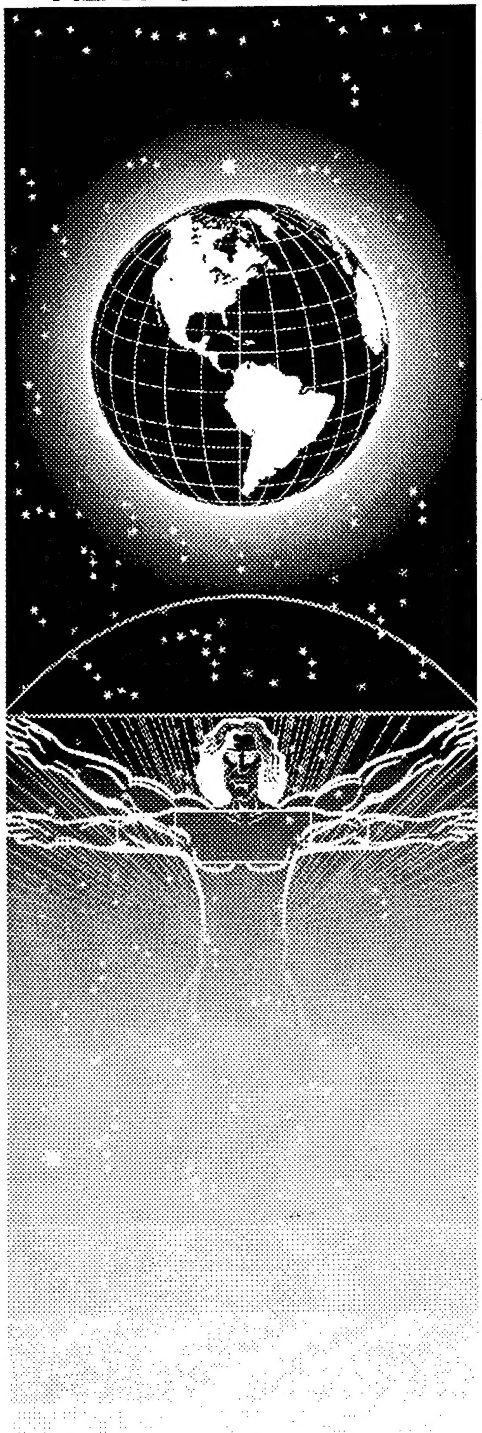

UNITED STATES AIR FORCE
ARMSTRONG LABORATORY

THE INNOVATORS: HIGH-IMPACT RESEARCHERS AT THE HUMAN ENGINEERING DIVISION, ARMSTRONG LABORATORY (U)

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FOR THE COMMANDER

Kenneth R Boff

KENNETH R. BOFF, Chief
Human Engineering Division
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There are several ways to benefit from lessons learned. One is to ferret out mistakes and issue procedures to prevent future errors. This study used a different strategy. We examined Armstrong Laboratory projects performed by successful researchers to try to find common themes that could be used to encourage more successes. Interviews were conducted individually with eleven scientists and engineers at the Human Engineering Division, Wright-Patterson Air Force Base. The participants were selected as a representative sample of the type of work the division wants to encourage. The participants were asked to describe a project that had a clear operational benefit to the Air Force. We obtained accounts of fifteen projects. Each project is described as a separate incident account, tracing the factors that contributed to the outcome. We identified several themes from the projects we examined. One theme was the skill of the researchers at problem finding; the researchers were adept at taking advantage of opportunities to pick challenging problems that were solvable. Another theme was the importance of colleagues and communities, particularly partnerships with user communities. Recommendations are presented for encouraging the initiative of laboratory personnel and increasing the opportunities for success.

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PREFACE

This project was initiated as a result of a concern that had been nagging at me since assuming leadership of the division in April 1991—almost four years prior to the start of this effort. At that time, it was evident that though the division had responsibility over a well-defined scope of human factors R&D, our resident expertise in most areas was typically but one deep. Moreover, we were locked into a hiring freeze while individual experts dropped out in retirement and the organization grew progressively grayer. This irreplaceable loss of corporate expertise was challenging in several ways.

Foremost was our concern for sustaining a corporate presence in key *product areas* where the Human Engineering Division had an historic presence based on significant contributions. Of almost equal concern was a gnawing fear and realization that this loss of key personnel irrevocably diminished the success-oriented intellectual capital and skilled critical mass vital to the operation of the unit.

As key individuals retired and departed from the division, few traces beyond formal professional communications remained to provide insight as to the factors which motivated them to success and the nature of the lessons they learned by experience which might be offered to future generations of human engineers following in their footsteps.

Fearing this loss of experience, I contacted Gary Klein who had previously conducted an innovative study at the Wright Laboratory at Wright-Patterson AFB to identify the contributing factors to the success of a distinguished group of managers, project engineers and S&Es. Intrigued by the product of this effort, I engaged Gary in a discussion as to how his "case-based" interview techniques could similarly be put to work to extract the underlying critical variables, heuristics for success, and lessons learned from the experience base of a selection of senior and retired S&Es from the Armstrong Laboratory Human Engineering Division.

This report documents selected high points from their successful careers and attempts to extract themes, attributes, and variables common to the case studies that are cited. Our hope is that this information can be useful as guidance to individual scientists, while also influencing the evolving processes, plans and operations of the Human Engineering Division.

Kenneth R. Boff, Ph.D.
Chief, Human Engineering Division

May 1995

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This effort was initiated by Ken Boff, who wanted to find a different way to tell the story of the Human Engineering Division in addition to bibliographies and historical reviews. We are grateful for his assistance in shaping the project and carrying it through. We also want to express our thanks to Lew Hann, Tanya Ellifritt, and Betsy Combs at Human Engineering Division for their roles in helping us move the project along.

We are also appreciative of the help given by the researchers we interviewed: Earl Sharp, Phil Kulwicki, Charlie Clauser, Don Topmiller, Dave Post, Bob O'Donnell, Mel Warrick, Julien Christensen, Steve Heckart, Jerry Chubb, and Lee Task. They were generous with their time in sitting through a two hour interview (and in some cases more than one interview), and in reviewing the written record to correct inaccuracies. More important, they were generous with their memories and impressions as they wrestled to figure out just how they had solved those problems.

We also want to express our gratitude to our colleagues at Klein Associates: Buzz Reed, Beth Crandall, Caroline Zsambok, Barbara Law, Mary Alexander, Debbie Goessl, and Diane Iglesias for their assistance in producing this document, including reviewing and editing the materials and making helpful suggestions for improvement.

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The Innovators: High-Impact Researchers at the Armstrong Laboratory Human Engineering Division

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The Innovators: High-Impact Researchers at the Human Engineering Division, Armstrong Laboratory

Authors: Gary Klein and Rob Hutton, Klein Associates Inc.

INTRODUCTION

This report describes a set of successful research programs at the Armstrong Laboratory Human Engineering Division. The goal of the effort was to conduct in-depth interviews with a very small sample of behavioral scientists and engineers at the Human Engineering Division, in order to formulate some hypotheses about how a researcher comes to have a strong impact on user communities in the Air Force.

For each of the interviews, we asked the researcher to discuss a project that had a clear benefit. "If the project was a story," we began, "the end of the story would be that the sponsors or users were enormously grateful and immediately benefitted from the findings or product. Give us an example that has this type of ending." Some of the interviewees came up with one or even several examples; others struggled because there was no clearly defined sponsor, even though the findings were valuable to a larger community. The interviews were therefore both challenging and engaging for the interviewers as well as the researchers.

Each researcher was interviewed individually. The interviews typically lasted for two hours. The researcher described a high-impact project, and we attempted to learn more about the decisions within the project, the types of critical judgments, and the problem-solving activities involved. We were trying to identify and understand a number of the characteristics of high-impact programs:

How were opportunities detected?
To what extent did the success depend on individual initiative?
What was the time course of the project, and of the benefits?
How were the researchers carrying out the strategic goals of the Division?
How did the researchers work through bureaucratic barriers?
What were the team dynamics of the effort?

Our objective was to identify common themes. In so doing, we were hoping to assist the Human Engineering Division in replicating these success stories. There are several ways that the Division can use these case studies. One way is to compile the lessons learned and begin to build a corporate memory to enable organizational learning to go on. A second way is to better understand the type of organizational support needed to increase the success rate. A third way is to use these case studies as models so that new scientists and engineers will have a better idea of what is expected of them. A fourth way is to consider organizational design issues that might increase impact.

There was another objective guiding this effort: to present a picture of the way researchers at the Armstrong Laboratory Human Engineering Division do their jobs. Technical reports do not always

convey the way the research was conducted—the frustrations, confusions, and satisfactions. In preparing the case studies we were seeking to provide a different perspective on how the Human Engineering Division works to support the goals and activities of the U.S. Air Force.

The case studies described in this report are not intended as a review of the work of the Human Engineering Division. Only fifteen projects are described, based on interviews with only eleven researchers. Clearly, the great majority of effective researchers and recognized accomplishments are not covered. The selection of people and projects placed an emphasis on senior researchers to try to take advantage of their perspective on the Division's activity. The majority of those interviewed are currently retired. The case studies cut across decades. The earliest are from the very beginning of the Human Engineering Division in the late 1940s, and the most recent are efforts that are still underway. Many changes have occurred during these fifty years. We have tried to identify factors that are time-independent, to show the working styles of researchers who have been able to make a difference.

The written accounts of the case studies are intended to convey the story line of the project, highlighting some of the themes. The reader may derive different conclusions than the ones we obtained, and we hope that our project descriptions are sufficiently clear to allow this type of constructive reading. Readers who seek additional information about individual projects are encouraged to contact the researchers themselves, or to review the relevant technical reports listed at the end of some of the stories.

CASE STUDIES

Honey, I Shrunk the Color TV: The Case of the Misleading Metaphor

An Interview with Dave Post

Sometimes we can become trapped by a metaphor. The idea of a miniaturized color display carries a compelling metaphor: a conventional color TV that just has to be reduced in size. But this metaphor contains a fatal flaw. The development of the Miniaturized Color Display depended on Dave Post's rejection of the dominant metaphor.

Post received his Ph.D. in Human Factors from Virginia Polytechnical Institute and started work at Armstrong Laboratory in 1983, in the Visual Display Systems Branch, working in the area of color research. Tom Furness was the branch chief. Post took the job because he liked the idea of helping to form his own programs; the jobs he had considered in industry paid more, but were restricted as to what topics could be investigated. Upon arrival at the laboratory, he enjoyed the freedom. He worked in the areas of helmet-mounted displays, and also performed some studies on the legibility of color symbology. After Post had spent a few years in the branch, Tom Furness asked him to try to build a color version of a miniature CRT, and he found it an interesting challenge, but the project ran into practical difficulties and was cancelled. Nevertheless, it did get Dave interested in the question of how to put color into helmet-mounted displays.

The Tradeoff

Color televisions come in all sizes, from giant screens to portables. However, there is a practical limit to how small they can be. The limit is based on resolution. When we reduce the size of a black-and-white CRT, the resolution suffers. But resolution degrades much more severely with color. The reason is that CRTs rely on three primary colors, red, green and blue dots, to form other colors. Each color is represented by different proportions of the three primary colors, and these fuse at a distance, appearing to be whatever color is being mixed. This works very well on a TV, with a big screen, seen at a far distance. It is not OK for a helmet-mounted display. The use of three primary colors translates into a 3:1 loss of resolution. It takes, on the average, three times as many dots on a color CRT to create the same picture on a black-and-white screen. When you reduce the size of the CRT to a few inches, the lost resolution makes the image too fuzzy to see.

As a result, designers have to trade off color for resolution. The 3:1 ratio means that a display has to be at least three times as large in a color version as in a black-and-white version, and even then the primary colors might not fuse adequately if the display is too close to the eye. This would seem to rule out the possibility of color versions of helmet-mounted displays.

Actually, for helmet-mounted displays, the problem is even more serious because these displays have to be useable even under bright daylight. The displays need lots of luminance, since the pilots have to be able to see the symbology against a bright sky. The increased luminance of the sky reduces the display contrast even more, making the resolution worse yet.

A Funding Vehicle Opportunity

Around 1987 Tom Furness began to think about using a new contracting vehicle, the Program Research and Development Announcement (PRDA) to fund some work to look at putting color in

helmet-mounted displays. The PRDA was non-traditional and was designed to encourage creativity and innovation. The idea is to state a problem and let vendors suggest the program needed. The traditional approach is to define the program and have contractors bid on it. Furness wanted to see how the PRDA would work, and he thought that the challenge of putting color into helmet-mounted displays was a good place to start. Under the PRDA concept, proposals were evaluated on the basis of the innovative ideas. Multiple awards were possible. The plan Tom and Dave drew up was to make three $600K awards for a two-year project. The first phase was just to demonstrate the feasibility of the technologies proposed. These would be evaluated to see which, if any, to continue to a second phase.

Seeing the Light

In preparing the PRDA package, Post reviewed the state of the art. What he found did not offer much encouragement. He confirmed his suspicion that the shadowmask design used in color televisions would not work because of the 3:1 resolution loss. Projection systems were too bulky to be used in cockpits. A field sequential, additive strategy threw away too much light, and suffered problems in which the color broke up (i.e., the primary colors didn't fuse in a satisfactory way). Post looked at lasers, but these suffered from poor efficiency. Lasers also had another difficulty—the laser images had no persistence, unlike phosphors. That meant that the images were going to be choppy and disorienting to the pilots.

While looking through the literature on color images, Post reread a book by Bob Hunt on photography. Hunt had worked for Kodak for most of his career, and had prepared a highly regarded work. Going through the chapter on color photographs, Post thought about the fact that color photographs are much sharper than color CRT images. The reason is that color photographs and slides don't use an additive method of combining red, green and blue. Instead, they rely on a subtraction method. They consist of three layers. Each layer filters out light at a different wavelength. The layers correspond to magenta, cyan, and yellow as the primary colors for subtraction. White light is filtered to remove portions of the spectrum. This works extremely well. The resolution of a color photograph or slide is about the same as a black-and-white image.

Post wondered if this color subtraction approach could be used with transmissive displays. He imagined how you could stack three Liquid Crystal Displays on top of each other to do the subtraction. **If this could be done, then he wouldn't have to be trapped by the color/resolution tradeoff**. He could get both good color and high resolution. Post knew that the technology wasn't out there to make this work, and he worried that the multiple filtering would cut luminance too much, but he tucked the idea away. It gave him confidence that there was at least one path worth exploring.

Soon thereafter, Post attended a meeting of the Society for Information Display. He talked to different vendors selling various types of displays, to try to drum up interest in the PRDA he was preparing. But he got the same response, "Well, I just don't see how you're going to miniaturize a shadowmask TV down to one inch."

> The idea of miniaturizing a color TV was such a powerful metaphor for a miniaturized color display that the vendors were trapped into the color addition strategy used by televisions. Their thinking was restricted by the metaphor.

Towards the end of the conference, Post ran into an old friend who listened to the description of the PRDA and introduced him to an engineer working for Sperry (in a division that later was acquired by Honeywell). Over lunch, the engineer described an earlier effort which carried out a color subtraction strategy. The engineer had set up a stack of LCDs, built a prototype, and patented it. The prototype was around four to five inches, and required direct viewing.

Post recognized that this was a great idea, especially since it matched the idea for color subtraction he already had formed. He had not wanted to impose this color subtraction concept on any vendors since it would defeat the purpose of the PRDA, to encourage a variety of approaches. Nevertheless, Post's confidence in the project went up now that he knew that the color subtraction method was more than a curious lead. He returned to Wright-Patterson and got the PRDA released. Many vendors submitted proposals, including Sperry/Honeywell.

Then everything shut down. The money for the PRDA was supposed to come through the Armstrong Laboratory's Supercockpit program, but that program disappeared. There was no funding for any of the proposals.

Running on Empty

That should have been the end of the project, since the rationale had been to explore the PRDA mechanism as a strategy for working with industry. The rationale was gone. And so was the funding. It was time to give up and shift to other projects.

But by this time, Post had become committed to the color subtraction strategy. He kept looking for alternative funding, and in 1989 a colleague in the branch, Dean Kocian, came up with $600,000. This would be only enough to fund a single effort, but that was enough. Post went through the process and reissued the PRDA in the Commerce Business Daily. He received around 17 proposals, which was more than he expected. He filled the trunk of his car with proposals on a Friday afternoon, and spent all weekend reading them. Honeywell submitted the only proposal based on color subtraction. Post satisfied himself that none of the other approaches was as promising, and by Monday morning he knew who should receive the contract. The rest of the procurement process proceeded, including gathering together an evaluation team and conducting formal assessment, a process that dragged out for about six months.

During 1991-1993 he monitored the contract. The prototype was as successful as expected. With the stacked LCDs, each pixel was a full-color pixel so there was no resolution penalty. Some team members worried that after the light passed through the filters, the display would be dim. This was not the case. To increase luminance, you just need a more powerful light source. Furthermore, the component on the helmet doesn't have to generate light (since you can use fiber optics and have a remote lamp), so there is no weight penalty. There were some concerns that the device would be too hot, changing the properties of the LCDs, but this proved not to be a problem. Everything went well. Except for one little difficulty. When the project was finished, there was no money for a Phase II, to take the project any further. Again, the project had run into a roadblock.

Fortunately, ARPA was investigating High Definition Television (HDTV) at the same time. One of the gaps in the ARPA program was small, helmet-mounted displays. Dick Urban at ARPA took on this task, and Henry Girolamo of the Army suggested that they set up a Tri-Service Advisory Group to guide the effort. This included NASA along with the three military services. The Tri-Service Group held several meetings, and included Bob Michaels and some other engineers from Wright Laboratories. Michaels already knew about Post's project, and feeling outnumbered by the other services, Michaels contacted Dave Post and invited him to attend the Tri-Service meetings.

Post accepted the invitation, and discovered that the group was considering conventional color filter LCDs and active matrix electroluminescent technologies. This was all well and good for ground-based displays but did not provide the required luminance contrast for airborne operations. Bob Michaels and Dave Post quickly convinced the group that they needed to rely on color subtraction methods.

In short order, the Tri-Service Group funded the Phase II project to follow on the work Post had been monitoring. Even better, Post no longer had the responsibility to carry out the procurement tasks of getting the contract awarded. The contract went to a team that included Standish (to assemble the LCDs), Honeywell (to assemble the subtractive stacks), David Sarnoff Laboratories (to design the circuit), and Kopin (to build the miniaturized circuitry). Post was the technical contract monitor. The Phase II carried out the original ideas and pushed them further.

Currently, the display has an active area of 1.3 inches by one inch. The resolution is 640x480 pixels, and a 1280x1024 pixel version is in the works. The miniaturized color display has the resolution and the luminance it needs. The Army has recently used the subtractive technology system for a color binocular helmet. The Air Force may use the miniaturized color displays in a follow on to its monochrome VCATS (Visually Coupled Acquisition and Targeting System) device. Additional applications of light-weight miniature color displays are being considered in medical endoscopy, entertainment (including HDTVs of the future) and virtual reality.

The Bomber Simulation, Test, and Evaluation Facility: The Million Dollar Meeting

An Interview with Earl Sharp

The Armstrong Laboratory at Wright-Patterson AFB has proudly been the owner of some of the most realistic simulation, test, and evaluation facilities for bomber crew stations in the world, thanks in large part to the early efforts of Earl Sharp.

Earl has just retired, but his legacy is the B-2 test and evaluation facility, the latest in the line of bomber simulators with sophisticated data collection capabilities for human factors research. The first of these state-of-the-art facilities was the B-52 facility which Earl set up in 1971.

Earl is a well-known character among the bomber crews, SPOs, and SAC. His research program for the defensive crew stations of the B-52, B-1, and B-2 are known to provide realistic mission scenarios for the purpose of testing and evaluating new concepts in crew station design, from the physical layout of the work station itself to such details as the design of the Synthetic Aperture Radar (SAR) target designation cursor (see, *The Case of the Fat Cursors: The B-2 Synthetic Aperture Radar - An Interview with Earl Sharp*).

This first facility came about through a series of accidents, such as a casual visit to a friend on base, a curiosity about some videotapes, and a chance meeting in an airline terminal. The final accident was when Earl decided to ask for $3500 in funding and walked away with more than $1M in support.

Just Visiting a Friend on Base

Earl's interest in the Electronics Warfare Officer's (EWO) workstation was sparked by chance in 1969. He had left his office with the typical comment "I'm just going to see a friend on base." When he got to his friend's laboratory, he realized he had come at a bad time, since his friend was still busy analyzing some videotapes of the B-52 EWO at work. Instead of leaving, Earl stayed to look at the tapes. Earl had heard about electronic counter-measures from his branch chief and was curious to see how the EWO actually did his job. He found the videotapes fascinating, watching the EWO stretching and reaching and jumping up to see hard-to-read instruments.

Earl asked if he could help with the videotape data analysis and became engrossed in the possibilities for human factors work on this crew station. He began to attend B-52 task force meetings addressing human factors issues in bomber aircraft.

And For My Next Wish ...

After sitting in as an observer in many task force meetings, trying to get an understanding of the human factors issues involved in the bomber crew stations, Earl had gotten to know the people and the issues. He had started toying with the idea of setting up a simulation, test, and evaluation facility to study the human factors issues in the EWO workstation and perhaps the others as well.

In 1971 Earl was invited to attend an Air Staff B-52 task force meeting at Oklahoma City ALC. He was waiting in the PAX terminal on his way to Oklahoma City and got to talking with the head of the task force about his ideas for a full mission simulation and evaluation device for human factors research. Earl was asked if he would like to present his ideas to the task force. Earl agreed, since this was a good chance to put his ideas into action.

He realized he would be asked for a budget estimate to go along with the program he was suggesting, and he didn't want to request too much for fear of looking greedy or making unrealistic demands. He also knew he was good at getting makeshift equipment made, so he decided that he could do the work for $3500. That's what he would ask for.

However, in talking to some friends just before the meeting started, he got some feedback that his $3500 estimate was unrealistically low. They told him it would cost at least a million dollars for the facility he would need. After juggling some numbers with a few people, and working up his courage, he came up with a figure of $270,000, which he put to the committee.

The task force was ready for his ideas. He walked out of the meeting with $270,000 in his pocket, and swears he could have told them the weather and the time of day and all they would have asked was, "When do you want the money?" Not only that, but he persuaded SAC to give him an old B-52 simulator on which he could base his facility, and two top-of-the-line simulator technicians to help him set it all up. All told, the value of the package was more than a million dollars, including the simulation facility, the technicians, and the funds.

In six months the facility was up and running with a little spare money to run some initial studies. The facility consisted of the simulator itself plus the capability of collecting performance data about every flipped switch, turned dial, and moved cursor. Earl could see what was pushed, when it was pushed, what it's starting position was, what it's end position was, how long it took to push it, and what the resulting input did. The crews were videotaped, as well as observed. They were debriefed using the videotape to try to understand what they were doing, and why they were doing it. The facility provided the capability to physically modify the workstation, as well as change the input devices and displays.

Earl had been a machinist before he had begun to get interested in human factors, so when it came time to start rebuilding and modifying the simulators to suit his needs he was able to go down to the local machine shop on base and tell the machinists exactly what he needed in their language. In this way he was able to get the facility set up extremely quick.

Putting the Simulation to Work

One of Earl's first projects was to redesign the workstation setup based on available anthropometric data that he had applied to the workstation. This project was spurred by his observations that the crews would sometimes not be able to reach or see important components of the workstation. Simply by applying an existing database of reach data with data about forces required to operate certain switches or knobs and dials, Earl came up with a more ergonomically sound workstation.

In order to test the new design concepts Earl recruited crews to run as subjects in highly realistic mission scenarios which consisted of real mission briefings, the mission itself, and the debrief, in order to provide the crews with the most realistic experience possible. At the time, SAC provided the details of the up-to-date threat types from Vietnam in order to challenge the crews and make them act as if they were really there. The crews reported that after about thirty minutes they had forgotten they were in a simulator, and also reported that the realistic missions provided them with better training than some of the simulator training conducted in the wings.

Earl's discoveries were sometimes frowned upon by SAC. Earl would discover what really went on in the aircraft rather than what doctrine dictated or what SAC thought was happening. He was able to guarantee the subjects' anonymity and confidentiality while they were being tested, so the crews acted as they would in the real aircraft. This brought credibility and respect to Earl's findings from the crews and from SAC.

> One example of this kind of discovery was evident from a study that utilized new threat types that intelligence had only recently released to Earl and to the wings. The wings had been given the threat type information to disseminate to the crews but, due to the amount of administrivia they were forced to do, the information was locked in a safe and a general announcement was made that the information was available. The crews never read the information. When Earl presented these threats some time later in an evaluation, the crews were not prepared to act appropriately. Some time after that evaluation, Earl heard on the grapevine that SAC had instructed the wings to concentrate on operational details above all administrivia. Of course these changes weren't attributed to Earl's evaluation findings, but they did occur very shortly after the evaluation. (SAC might give you a different story.)

A similar facility was subsequently set up for the B-52 offensive workstation, and B-52 flight deck, with similar data-collection capabilities.

The Next Generations

In 1983, SAC asked Earl's group to build a simulator facility for the new B-1 bomber which was under construction but for which there was no training device as yet. He built two simulators for them to serve as interim trainers while the production trainers were being produced. While these two simulators were being fielded Earl built another, higher fidelity, defensive simulator for the B-1 and they began a similar operation for the B-1 as he had for the B-52. Subsequently, the two fielded units were returned to the lab and served as an experimental platform for all B-1 crew positions. Earl's group is currently working on similar devices for the B-2, which is to be Earl's legacy to the Armstrong Laboratory.

Earl's work has always been highly regarded by the crews, the SPOs, SAC, and the Armstrong lab. His approach to his job is based on being able to understand and, in some cases, do the job of his user group. This has required hours of sitting with the crews, working with the crews, talking to, and debriefing the crews: getting into their heads and being one of them. For this he has been rewarded with the trust and respect of all. In return he has provided the Air Force with state-of-the-art research and testing facilities as well as numerous modifications which have been implemented in the aircraft modification.

Earl's keys to success? Keys to overcoming the stumbling blocks and barriers, to showing initiative? He claims he came across few stumbling blocks. The reason may have been because of his self-starter attitude of avoiding going to high level help, taking responsibility for the success of his own projects, and being given the freedom to avoid getting caught up in the red tape that it requires to get things approved in advance. He has found it is easier to ask forgiveness than permission, but he has earned that privilege from years of dedication to the betterment of the bomber crews' workstation, and from the success of his endeavors.

Earl estimates it took him about two years to be productive. Two years of active data analysis, sitting in front of videos of the crews performing their day-to-day, minute-to-minute tasks. Two years of talking to the crews in front of the video, asking them what they were doing, why they were doing it, what had prompted them to do that. This active data analysis was key to understanding the domain and later being able to make informed choices for test, evaluation, and modification. This close interaction with the crews and "getting into their heads" is seldom done by short term contractors and is also difficult for the Armstrong lab people to do nowadays.

Summary

What if Earl hadn't been around? Would there have been a longstanding simulation and testing facility developed for the bombers? Would there now be a high fidelity, sophisticated data collection and human factors research device for the B-52, B-1, or B-2? Are the crews flying in safer aircraft with more effective control panels and displays? Earl wouldn't stretch it that far, "You can't say, because the 'successes' have never been tried any other way; so you can't test the alternative. But many of the changes have been made on the drawing board rather than with the welding torch."

The Case of the Fat Cursors: The B-2 Synthetic Aperture Radar

An Interview with Earl Sharp

In 1969 Earl Sharp chanced on an old friend looking at some videotapes of the defensive workstation of the B-52. This fortuitous encounter led Earl to take an interest in the Electronic Warfare Officer's (EWO) workstation which has led to a 20-year commitment to improving the design of the crew station in the latest Air Force high tech bombers, including the B-1, B-1b, and currently the B-2.

Earl has been responsible for the development of some of the most realistic bomber simulation and testing facilities for human factors research in the world. Having persuaded the powers-that-be in a task force headed by Pentagon Air Staff and made up of Strategic Air Command, Air Force Systems Command, several Air Logistics Commands, and many Air Force engineering organizations, to give him $270,000 and a B-52 training device in 1971, Earl set about putting together a high fidelity simulation facility. The facility had the capability to collect performance data from every thrown switch, twisted knob, and CRT display. This facility has since produced valuable data which has provided the impetus for many crew station and display design changes for the B-52, as well as being the template for a similar B-1 facility, and Earl's legacy, a B-2 test and evaluation facility.

Earl's philosophy has always been to put the aircrew member in as realistic a scenario as possible and see what he really does. This required the development of a realistic simulator with realistic scenarios. Earl has become so respected in the bomber community that he is trusted with the most up-to-date threat types for his simulations, and is provided with the means to sit in the wings and talk turkey with the guys to whom he dedicates his work. This respect is carried through from the crews, to the SPO, the Human Engineering division, and Strategic Air Command.

During a project, daily, and sometimes nightly, communications are set up with the friends he has made in these organizations in order to talk about the problems and issues at the heart of the problem, and come up with solutions that will make a difference to the guys that fly the missions.

> Earl has become so familiar with the domain that the avionics group at SAC HQ required that all modifications to the defensive workstation go through Earl's group before it go into the aircraft. The avionics group believed that Earl knew the domain better than they did.

Being able to put himself in the crew's shoes has enabled Earl to develop an empathy and understanding of the aircrew member's task, and therefore to come up with realistic and functional redesign solutions to the problems given to him by the SPO. The latest problem tackled by Earl and his group was presented by the SPO as a target designation accuracy problem due to the thickness of the cursor in the Synthetic Aperture Radar of the B-2.

The Case of the Fat Cursors

In December 1993, Sharp and his colleagues received a service report from the Crew Test Force (CTF). The service report outlined a problem with the Synthetic Aperture Radar (SAR) cursor. It was too fat for the aircrew member to position accurately. The aircrew member needed to be able to position the cursor within one pixel of the target, but the cursor was three pixels wide so it would sometimes obscure the actual target. The CTF and B-2 SPO thought that the solution was probably to make the cursor only one pixel wide, but wanted Earl to evaluate a solution to demonstrate performance improvements. Earl's team flew into action. They began to get the facts about the cursor, they talked to the crews, the CTF, and commiserated with the SPO over the phone. Earl's

> This particular service report came without prior notification, but Earl keeps his ear to the ground when it comes to potential problems. Regular contact with the B-2 SPO, the CTF, and the crews by phone and by being on the e-mail circuit enables him to keep up with the latest developments and problems as they arise.

team

uncovered a number of other issues. The crews had also reported problems with the mechanization of the cursor and the positioning thumb controller which contained delays and deadband which hindered cursor positioning.

Having brainstormed to produce as many alternative designs as possible, they narrowed the options down to a testable number and began to prototype them on the system to see what they looked like and how they worked. The goal of the evaluation was to prove that an alternative design would provide solid performance advantages over the existing cursor design.

> One of Earl's jobs is to help the SPO redefine the problem. The human factors knowledge combined with an intimate understanding of the crew and their use of the SAR enabled Earl and his team to think about the cursor not merely as a cross on a screen but as a pointer that must be directed quickly and accurately within a cluttered radar display. This provided Earl's team with a unique view of the cursor that was not so clearly understood by the SPO or CTF, enabling them to come up with useable solutions. In this case there was not enough time to deal with the positioning mechanism, but Earl pointed this out as another issue to be resolved in the future. Earl always likes to give more than he's asked for on a project.

Earl's ability to know what the crews will want is based on his team's vast experience base and cooperation with the bomber crews. His team has specialized in the defensive aspects of the bomber's task. A navigator colleague, Jim LaSalvia, provided expert knowledge about the SAR system. Earl knows the aircrew member's job, the day-to-day and minute-to-minute tasks, the equipment, the people.

> With the potential of missing alternative designs, Earl's philosophy is that it's easier to eliminate options once they've been generated than to generate new ones later in the process.

Towards the end of January, the options had been narrowed down to a useable number, and the team was about to start testing in a few days' time when they discovered that what they assumed to have been a black-and-white cursor on a green SAR screen turned out to be a black-and-white cursor on a black-and-white screen. All the previous versions of the SAR that the team had seen were green screens.

The whole problem had changed, now that they realized the cursor was too small and got lost in the clutter of the screen imagery. Not only did they have to make it thinner, now they had to make it more easily seen and less easily lost. A rapid brainstorming session took place and new candidates were suggested and rejected or accepted based on accuracy of placement, and now also conspicuity. Two candidate cursors were kept for evaluation against the original cursor by the crews using simulated displays (see Figure 1).

> Earl's team brainstormed with the CTF, the B-2 crews, and the SPO in order to come up with potential redesigns. This process is vital to the generation of as many options as possible, and to the development of a team understanding and a mutual responsibility for and investment in the end product.

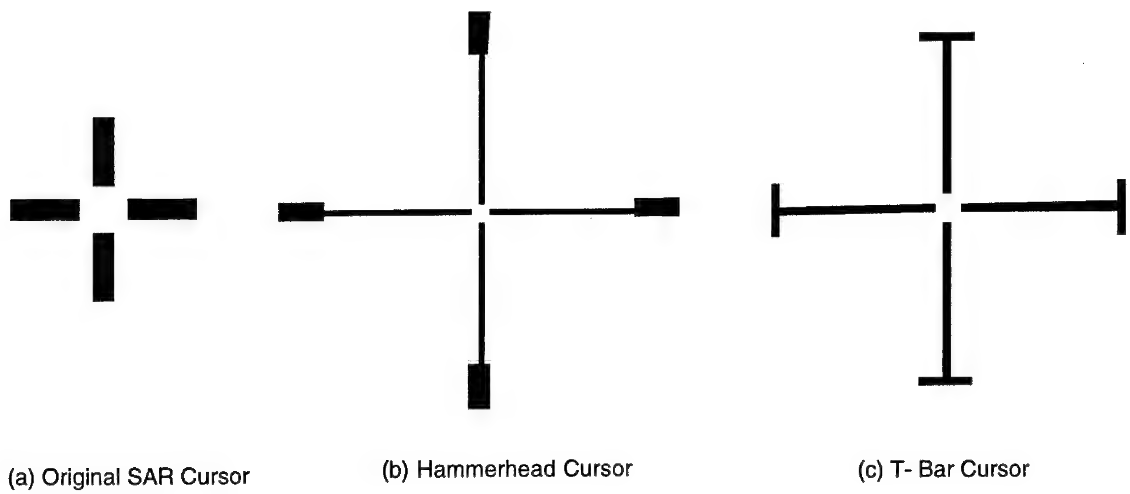

Figure 1. The three cursor designs used in the evaluation.

By February the study was ready to be run. The candidate cursors were incorporated into the simulator in a realistic target designation scenario. Subjects with SAR experience were recruited and run through the simulations using the three different cursor types.

The crews were more than happy to help Earl out in evaluating a new design. The simulations he used were more challenging and up-to-date than some of the training in the wings. Earl's reputation and rapport with the crews has enabled him to rely on them for their support in running his studies.

By March the evaluation was complete and Earl was able to present the results. The T-bar cursor had provided the clearest advantage in positioning accuracy and conspicuity. Within two to three weeks of the end of the evaluation Earl was able to provide the preliminary results.

The temptation at this point may have been to present the data to the crews since they were the primary user. However, Earl knows the system: Don't bite the hand that feeds you! His first presentation is always to the primary sponsor of the project, in this case the SPO, followed by the other interested parties including the CTF, operational command, ACC, and the bomber wings themselves. Often nowadays these presentations are to multiple interested parties simultaneously.

The timing of the presentation is also a critical issue. Even though Earl wants to get the information to the SPO and crews as quickly as possible in order to meet the modification deadline, he wants to make sure his data are solid and reliable before he presents them.

A technical report was also written by Earl's team, describing the evaluation, and submitted to the Armstrong Laboratory (AL/CF-TR-1994-0020).

The modification to the SAR, based on the recommendations from Earl's study, were reviewed by Northrop for engineering considerations and equipment compatibility. Several months later they were incorporated in the next modification of the B-2. An appreciation for the acquisition cycles and stages of development is needed in order to be able to bring the project in under the deadline that precedes the preliminary design review.

With the exception of the tech report the project was informal in that there was no administrative review from AL, no documentation or reports other than Earl's own paper-trail of memos and messages. These kinds of informal interactions defy scheduling. They permit rapid information gathering and give-and-take without going through lengthy administrative red tape.

Earl's primary concern is for the bomber crews and he'll do whatever he can to support them and help them in any way. His perseverance in the face of adversity is based on his discovery of an enthusiastic, imaginative, and unique group of users who were eager to be recognized and listened to. Earl saw this opportunity and it has led to a 20-year relationship that has fueled his efforts and produced numerous successes in work and friends for life.

One reason why the SPO relies on Earl and his team is that they know he'll get the job done, and done on time. They've worked closely together before, trust each other, and know each others' turn-around times. If Earl had not been able to act quickly to analyze and present the results within the time cycle of the modification, money would have been wasted later for an on-line modification or the new cursor may not even have been implemented until a later modification.

Summary

The SPO had suggested a simple fix to the fat cursor problem: Make it skinnier. They wanted Earl to provide the performance data to validate the change. In assessing the problem, Earl's team had redefined the problem and come up with workable solutions to the real problems faced by the crews when using the SAR cursor. Not only was the cursor too thick to accurately designate a target, but if it was merely made thinner, it would also have been difficult to find it in the clutter of the display.

> The domain knowledge and understanding of user requirements held by Earl's team enabled them to imagine the implications of the different cursor redesign options. They were thus able to provide realistic alternative cursor designs for evaluation and implementation.

Earl's team saved the SPO time and money by identifying the critical issues, evaluating only workable alternatives, avoiding the implementation of an unsatisfactory cursor, and avoiding the necessity for a further modification. Had the small skinny cursor been implemented, the crews would have had a harder time successfully completing their missions. Eventually the complaints would have mounted up and a further fix would have to have been made.

Flying Ahead of the High-G Cockpit

An Interview with Phil Kulwicki

Applied research is sometimes like hitting a moving target. You have to aim where you think the target is going to be, rather than where it is. Phil Kulwicki was able to anticipate human performance problems with the high-G cockpit, and prepare solutions before they were needed.

The Other Side of Gravity

Kulwicki started working on the human performance implications of the high-G cockpit in 1969. Prior to that time he had been on the other side of gravity. After graduating from the University of Detroit in aeronautical engineering in 1964, he began working in the Armstrong Laboratory's Crew Stations Branch as part of its zero gravity research program. During the 1960s, the Branch was investigating man-in-space issues as the era of spaceflight dawned. Important parts of the work involved flight testing (using C-47, C-131B, and KC-135A test aircraft) where various human performance experiments were performed in the airplane's large cargo bay. During that time period, the Laboratory also supported NASA's man-in-space program by providing zero gravity familiarization training for the astronauts, and Phil served as one of the instructors. This was the astronaut's first introduction to zero gravity "free-floating," experiencing weightlessness for 25-30 seconds during the airplane's parabolic maneuver trajectory.

After completing the Master of Science curriculum in aeronautical and astronautical engineering at The Ohio State University, Phil rejoined the lab in 1968 at a time when the Air Force investment in R&D for manned space applications was reduced as the work transferred to NASA. Therefore, he turned his attention to understanding how new design technology would affect aircraft pilots.

The Challenge of High-Performance Airplanes

In 1969, Kulwicki got a new boss, fighter pilot Major Dick Ravenelle. Kulwicki and Ravenelle were drawn into discussions about a puzzle. Between them, they covered the pieces of aeronautical engineering, human performance, recent technology, and fighter tactics. The puzzle they were trying to solve was how pilot performance was going to be affected by the new high-performance capabilities of the next generation of fighter aircraft, typified by the highly maneuverable F-15 and F-16 fighters.

These tentative explorations set the stage for the next quarter of a century in Kulwicki's career with the Armstrong Laboratory, which combined laboratory research in crew station design, contract research with most of the major aircraft and avionics companies, and applications of state-of-the-art technology to push the limits of the pilot-aircraft system.

The F-15 Eagle could sustain 9 Gs in a level turn at combat weight. Previous generations of fighter aircraft had neither the airframe design nor the engine power to operate in such an environment. How well would pilots withstand such forces and how well could they perform? Before addressing those issues, the first puzzle was whether this was a problem worth considering. Just because the F-15 could sustain those forces did not mean that pilots would be pulling 9 Gs in combat. There is no sense in solving a problem that might not be real.

The Problem is Real

Kulwicki and Ravenelle worked on the high-G cockpit research program from 1969 to 1972 as an unfunded in-house project. They didn't receive much in the way of laboratory funds, but they could devote their in-house labor to the project. The task they set themselves was to estimate whether pulling high-G turns in aerial combat provided much benefit. They figured that if the benefit was great enough, pilots would be crazy enough to maneuver in that G-force envelope. And that would mean that they better start thinking about how to provide sufficient G-protection.

In order to continue Armstrong Laboratory funding, they had to find a "customer" for the project, an organization outside of the Human Engineering Division. Kulwicki and Ravenelle contacted the appropriate people at the Aeronautical Systems Division at WPAFB, in the development planning community. There was agreement, given the new cockpit concept's intuitive appeal, that the previous design philosophy (of conventional fighter maneuvering load limits) was too restrictive and that the high-G cockpit could be a key to the new design freedom.

In order to help justify funding by Armstrong Laboratory, ASD issued a formal Technical Need document. Getting the technical need was no easy feat, but after eight months of discussions and selling the idea, the Technical Need was issued in 1970. The Technical Need was highly regarded by ASD, and the laboratory's work was given high importance ratings and high progress ratings in the annual Technical Need reviews. The Technical Need also proved to be instrumental in gaining and sustaining industry interest in the new concept. For Kulwicki and Ravenelle, getting the Technical Need issued meant they had the freedom they needed to study the puzzle of whether high-G maneuvering would result in tactical advantages.

> The discussions, collaboration, and cooperation with ASD were instrumental in achieving the tech need. ASD's input was sought as to how the project should proceed, identifying the boundaries of the work, and convincing ASD that the pay-offs and benefits would be what they claimed they would be. The process of obtaining the tech need was considered to be much smoother because of the identification of a user-group at Wright-Patt.

At that time, digital computer models of relative aircraft maneuvering were being constructed, and Kulwicki and Ravenelle decided to use such a model in building the case for a high-G cockpit. The Rand Corporation had developed a computer program that was named TACTICS II after its intended use as a simulation tool to help analyze the tactics of air combat. After contacting the Rand developers, they obtained the TACTICS II model for free, and installed it on a laboratory computer. The team got some more free help, because they were joined during this period by Captain John Lyons, a graduate electrical engineer from AFIT. They began to analyze the potential operational payoffs of flying high acceleration maneuvers.

The laboratory team used the Rand computer model to estimate the increased combat effectiveness of next generation fighter aircraft, the F-15 and F-16. That was done by simulating air-to-air maneuvering dogfights with a range of aircraft, varying the airplane's structural limit load factor. The aircraft were "flown" against one another in a large number of separate air combat engagements, using missile and gun firing opportunities as measures of success. Typically, one of the aircraft was programmed with maneuvering capabilities above the previously accepted pilot G-tolerance limit of 7.5 Gs, and the resulting (maneuver and combat) superiority over its adversary was demonstrated by achieving weapon firing opportunities earlier in the simulated engagement. Even after paying the price of extra weight for the aircraft structure needed to increase the maneuver load factor, Kulwicki, Ravenelle, and Lyons demonstrated clear evidence of the

projected operational payoff. The first part of the puzzle was solved. By moving above the G-tolerance limit of 7.5 Gs, pilots would gain a clear advantage.

The first step to building the credibility of the research program was the use of the Rand air combat simulation, TACTICS II. Despite factions in the engineering community that were skeptical of the potential benefits of using computer models to simulate dynamic environments, Kulwicki, Ravenelle and Lyons found a testbed to show the feasibility of their ideas about high performance aircraft, . . . and the potential for a high acceleration cockpit.

The team presented their results at a classified symposium in 1972. Immediately, the work caught the attention of the fighter development community in both the Air Force and industry.

The work was later extended in an AFIT master's thesis by Major Dean Vikan, which involved the parametric evaluation of a family of aircraft designs built around the high-G cockpit concept, using the same TACTICS II software. These computer projections of capability provided the initial ammunition with which to sustain the ASD's Technical Need, and to help to justify the subsequent laboratory funding for concept development.

Kulwicki had also arranged for some converging evidence. After the Technical Need was issued in 1970, Kulwicki arranged for the first R&D contract in the project, a contract with the McDonnell Aircraft Company (MCAIR). At the time, MCAIR had just developed a high- fidelity flight simulation facility with the then state-of-the art scene generation capability. Such an engineering simulator can be used to rigorously evaluate the aircraft and cockpit during development, and was well suited to study air combat. MCAIR's advanced engineering project staff was also working on F-15 design upgrades for ASD. MCAIR's initial contract work involved both cockpit design layout (i.e., design and installation within vehicle engineering constraints) and new estimates for projected combat payoff using its computer simulation model resembling TACTICS II.

Can Pilots Fly the High-G Maneuvers?

Once it became clear that high-G maneuvers were effective, the second puzzle was how to help pilots fly these maneuvers without blacking out. The F-15 cockpit uses a conventional, upright ejection seat and a bubble canopy, giving the pilot the access to good external vision that is needed for air combat. However, even with G-suit protection, the upright seated position also makes the human most susceptible to blackout from the high G-forces that are available at the pilot's command. The G-suit protected pilot was known to be able to withstand about 7.5 Gs. However, the advanced F-15, once it had spent part of its fuel, could pull 9 Gs at the drop of a hat.

The F-16 was another case. Despite the initial claims of improved G-tolerance in the F-16, with its 30 degree seat reclining angle, subsequent centrifuge data proved that the seat recline didn't add much. The seat was only more comfortable, to the point of black-out! From these studies emerged an extension of the G-tolerance database which serves now as a reference for cockpit and aircraft design.

The Articulating Seat

Kulwicki began to wonder what would happen if the seat could be reclined back even further. An "articulating seat" was designed which allowed the pilot to change the angle of recline during the operational mission, such as upon entry to a combat zone. The seat could be set more upright at take-off and during routine aspects of flight, for example, and reclined back to an optimal angle in cases where maximum G-tolerance was required. This would expand the pilot's G-tolerance (and ability to think and react). The articulating seat actually raised the pilot's torso and legs rather than

simply lowering the backrest. This was done to preserve the important out-of-cockpit vision which is paramount for air combat. Also, by using an electromechanical insert to the conventional ejection seat they could avoid the cost of developing a new ejection seat.

MCAIR's work was so promising that a sole source follow-on contract was established with MCAIR in 1973 for man-in-the-loop, interactive combat simulation and to build and deliver a variable geometry, articulating seat to investigate the human G-tolerance under actual G-forces in the centrifuge. A third contract was awarded to MCAIR in 1974 for a follow-on air combat simulator evaluation. Highly experienced fighter pilots, all fighter weapons school graduates or instructors from the Air Force and Navy, "flew" hundreds of one-on-one air combat engagements in the MCAIR simulator, providing another layer of data testifying to the concept's promise. The centrifuge seat was first used at Brooks AFB for proof testing and later installed at Wright-Patterson AFB for additional research in human G-tolerance as a function of seat position.

Building on that work, additional contracts were awarded to MCAIR (for the F-15) and to General Dynamics (for the F-16) from 1974 to 1976, to work out the installation details for a prospective flight demonstration. Design issues were worked out, such as the placement of hand controls, impact on cockpit display panels, ejection seat concepts, and related engineering design factors. Other airframe contractors began to study the concept with their own funds. From 1971 through 1974, Kulwicki and Ravenelle visited every major military fighter maker in the country to discuss the project and share results. Thus, industry was also developing the engineering design data needed for future applications. Preparations were nearly ready for a flight demonstration.

In 1974, following the successful work conducted in support of the ASD tech need, the Flight Dynamics Laboratory at Wright-Patterson AFB became convinced that the work could turn into an advanced development (6.3) project. At the time, Armstrong Laboratory did not have advanced development projects, but the Flight Dynamics Laboratory did and they became interested in high-G cockpit flight demonstrations on the F-15 and F-16. Demonstrations using both aircraft were planned. This led to a profitable collaboration with Wright Laboratories, with Kulwicki at the Armstrong Laboratory providing technical guidance and Ravenelle moving to Flight Dynamics Laboratory to coordinate the project's transfer.

An advanced development project was approved for Flight Dynamics Laboratory in 1976, and considerable engineering was performed. But this part of the story ends on a disappointing note. The actual flight demonstrations were not performed when funds for the flight test part of the advanced development were removed.

Better Training and Equipment

A further outcome of the Laboratory's interest in pilot performance in high-G environments was an increased awareness of the importance of the design of personal equipment such as G-suits, oxygen masks, and of pilot G-tolerance training. This heightened awareness may have spurred the successful research program at Brooks AFB, which later developed personal protective equipment to support the pilot in high-G environments. At the time of the high-G cockpit project, researchers at Brooks AFB were developing the M-1 straining regimen that pilots use to boost their G-tolerance, and were using the human centrifuge to train fighter pilots to detect the signs of impending blackout. The high-G cockpit work preceded those separately developed advances in life support equipment, such as the use of chest counterpressure and positive pressure breathing. These investigations all led to the engineering development of the Combat Edge equipment, which has been fielded in our fighter aircraft. This gear helps to maintain blood in the head and upper body to forestall blackout.

Summary

It is likely that, without the work conducted at the Armstrong Laboratory by Kulwicki and company, the state-of-the-art for crew station design, pilot equipment, and pilot training for high-G environments would not have matured as rapidly as needed, to keep pace with the emerging breed of highly agile fighter aircraft. Their persistence in obtaining the objective engineering data proved vital to the credibility and feasibility of the project, especially in the early days. The safety of pilots and overall system effectiveness have been greatly enhanced because the issues had been anticipated and studied.

Targets of Opportunity: The Case of the Corrupted Cues

An Interview with Phil Kulwicki

"Fighter aircraft exist to destroy other aircraft. The airplane itself may be considered only a weapons platform designed to bring the weapons system into position for firing.... All requirements of a particular weapon must be satisfied simultaneously in order for the weapon to be used successfully."
(Shaw, 1985, p. 1)

"The air-to-air gunnery problem is a difficult one; it involves hitting a moving target from a moving platform with projectiles that follow curved paths at varying speeds." (Shaw, 1985, p.7)

During times of revolutionary change, it is easy to overlook parts of a system that have worked well in the past, but will be inadequate for the future. This was the problem when the high acceleration fighters, the F-15 and F-16, came onto the scene. The cues about when to shoot changed dramatically. Fortunately, Phil Kulwicki, and his colleagues at the Human Engineering Division had been anticipating these difficulties and were ready with solutions.

High Angle-off Shots

In the past, fighter pilots had to judge their firing opportunities using their skill and experience, taking into account multiple characteristics of their own weapons, their own flight path and the maneuvering of the enemy. With the progression of technology, gun and missile firing opportunities are now calculated and cued using algorithms that take this information into account. When a valid shooting opportunity is calculated, a "shoot" cue is displayed to the pilot on the HUD indicating a high probability of a hit if the weapon is released at that point in time.

To aid in the art of gunnery, the Lead Computing Optical Sight (LCOS) was developed to reduce the mental calculations and predictions required by the pilot to hit his adversary. However, with the advent of low wing loading, highly agile aircraft such as the F-15 and F-16 in the mid-sixties and early seventies, the assumptions on which the LCOS prediction algorithms were based became obsolete. Rather than shooting at the target from its "six" at 1 G, the pilot was now having to take fleeting shots at high deflections from the enemy aircraft's nose or tail. The algorithms were not designed to compute the required information for these high angle-off shots. The LCOS was both providing invalid shoot cues and missing some valid shooting opportunities. Even small discrepancies from the algorithm's assumptions could result in improper firing cues. This problem was not confined to air-to-air gunnery, but also applied to firing air-to-air missiles, a problem which was later addressed by Kulwicki's group.

Phil Kulwicki and his colleagues, particularly Dan Eliason, at the Armstrong lab became interested in this problem since it also became a concern for both the Air Force and Naval air combat communities. A system that provided shooting cues that were based on invalid assumptions and were poorly displayed to the pilot was operationally and economically detrimental to both communities.

The issues involved were not only problems of prediction but also problems of displaying the information to the pilot. This information display issue particularly sparked Kulwicki's interest and led to a three-year effort between 1977 and 1980 to try to improve the fire control systems in the next generation of agile aircraft. The result of the effort was improved HUD symbology to accompany the recalculated algorithms, thus improving the combat effectiveness of the F-15 and F-16.

Finding the Problem

In 1977 Kulwicki attended a symposium in San Diego at which the Air Force and Naval aviation communities discussed issues concerning the evolution of air combat. This conference planted a concern for display issues relating to the cueing of valid firing opportunities based on new assumptions for highly agile aircraft. No one at the conference appeared to be tackling the problem, so Phil began to bounce around ideas about the problem in terms of the pilot-aircraft interface with Eliason and others in the lab. Eliason, Phil's branch chief, was influential in the early stages with his background as an F-105 pilot and his interest in pursuing the idea as a potentially high pay off project for the lab. Kulwicki had also been in contact with the Avionics lab at Wright-Patterson, which had already been involved in developing fire control cueing algorithms, and therefore had some technical knowledge of the problems which would have to be faced.

Phil recognized an opportunity to combine the Avionics lab's knowledge of the algorithm problems with his own interest in the display issues relating to human performance and began to put together his ideas for a proposal for the AL Annual Program Review Board.

> Kulwicki was already involved with the fighter aircraft community after the AL's involvement with developing crew station design concepts required for improved pilot performance in high-G environments. This involvement had forged contacts which allowed Phil to keep his ear to the ground about potential crew station and display design issues that were being faced by the pilots. A further important result of this earlier work was contacts with the aircraft manufacturers. The ongoing involvement of these industrial contacts was invaluable for helping Phil establish the credibility of the lab's design concepts and getting them implemented into the aircraft, rather than having them gather dust as a tech report.

The Lab Mission

At this time Phil had just completed another project and had some time to pursue other projects that caught his interest, so, following the Air Force/Navy symposium, he submitted his ideas about the gunnery and missile cueing problems to the Annual Program Review. The work that he proposed fell within the charter of the lab and had already been identified as operationally relevant by the air combat communities. The problem had also been bounded by the operational communities and the other players in previous related efforts, such as the Avionics lab who were interested specifically in the calculation of the algorithms which would drive the display elements.

> Previously, in 1975, Phil had written a White Paper which outlined a new lab initiative towards improving crew station design in the new wave of fighter aircraft. The Air Force had envisioned the obsolescence of the F-111 and F-4 to be replaced by aircraft, such as the F-15 and F-16, with capabilities that would stretch the pilot to the limit, both physically and mentally. Phil proposed that the Armstrong lab should become involved with looking into the man-machine interface issues that he recognized would affect, and were already affecting, the operational effectiveness of these new fighter systems. Thus, when the fire control problem proposal was raised at the annual review, the lab had already been primed to accept this type of project and were prepared to back it with funding.

Assembling the Team

> The proposal of such a project involving multiple players was somewhat unusual within the Armstrong labs at that time. To be so proactive in finding issues that were not only interesting from a research and development perspective, but were also operationally relevant was not a common occurrence within the lab. Contacts forged within the air combat community through previous work allowed Phil to identify potential opportunities for such research.

Phil's proposal included the need to interact with industrial contacts in order to sensitize them to the fire control cueing issues and to aid the transition from lab research to the actual aircraft: lessons learned from previous projects including the high-G cockpit.

Display Concept Development and Evaluation

In the proposal Phil had outlined an approach utilizing both low- and high-fidelity dynamic simulations. Pilots would interact with the simulations and give their judgments of when a good firing opportunity occurred and weapon effectiveness was scored. This was in sharp contrast to similar efforts in the past which had employed static pictures of the different fire control displays in order for the pilots to base their judgments of valid firing opportunities. With funding provided by the Avionics lab, Phil arranged for the Hughes Aircraft Company to develop and evaluate the new fire control displays using the dynamic simulations.

> The use of dynamic displays as a means of studying how firing opportunities were assessed was also a major step up from the use of static picture representations of the combat situation. The use of this kind of technology at that time had not been fully embraced and the decision to use these techniques was not without question within the lab.

Based on the pilots' descriptions of what information they would use to calculate a valid firing opportunity, the lab was able to produce display concepts which enhanced the pilots' ability to interpret and act on the fire control cues. The fact that the displays were dynamic enabled the lab to discount artifactual cues from the static displays which had been utilized in previous efforts.

During 1978 these display concepts were programmed into the McDonnell-Douglas and General Dynamics simulation facilities for test and evaluation. In 1979 they were evaluated in a high fidelity simulator. The new fire control system, including both the improved algorithms and the new display symbology, was found to improve the weapon system's effectiveness.

SPO Involvement

The display recommendations were handed off to the SPO and subsequently to the SPO's contractors, the companies involved in the development and evaluation phases of the project. Eventually the improved fire control systems were implemented in the F-15 and F-16, including the new computer algorithms and improved displays for gunnery and missile control.

The SPO had been involved from early on in the project. This involvement avoided later complications and misunderstandings about the key decisions that were made in the design process. Kulwicki had also ensured that the SPO's contractors, McDonnell-Douglas and General Dynamics, were brought in on the project to work with the AL so that when the final stage of implementing their ideas came about the key players were already in place and up to speed.

Summary

This relatively short project for Kulwicki and colleagues resulted in identifying a problem faced by an operational community, seeing the display implications that were part of the problem, and producing a new display configuration that improved operational effectiveness in the new wave of USAF fighter aircraft.

In addition to the obvious impact on enemy kills and subsequent survival rates of American air crews, the work had an economic impact by curtailing the likelihood of wasted shots of expensive missiles fired "out of launch zone."

Many basic and applied research projects are designed to carefully work on a problem for several years before transitioning it to the next step in the development cycle. Here, Kulwicki was able to respond rapidly to an Air Force need, and to use his networks in industry and in the SPOs to quickly inject the improved symbology.

In a way, Kulwicki was taking a high angle-off shot at a research target. He quickly noticed the opportunity and rather than engage in a lengthy tail chase, he was able to lock on to the source of the difficulty and launch a solution within a time frame that ensured impact.

Reference

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On Top of the World: The Case of the Confusing Compasses

An Interview with Julien Christensen[1]

One of the challenges facing human factors professionals is to take what they can see and convert it into data that will convince others. This incident describes how Julien Christensen responded to that challenge by trying to improve the navigator's tricky task of navigating in the polar regions.

This incident took place early in Christensen's career, around 1947. After World War II, Chris went to work with Paul Fitts, the father of human engineering in this country. Fitts was the director of the Engineering Psychology Branch, in the Aero Medical Research Laboratory. During Chris' Air Force career he had been an aerial navigator and a radar bombardier. This background was one that Fitts felt he could use.

Arctic Navigation

"The Air Force was doing reconnaissance work in the Arctic (photographic, and weather reconnaissance). They were having lots of difficulties with their navigation because most of the time the airplanes were too far up to home on the north magnetic pole[2] on Prince Edward Island, north of Canada. When you were in that region, the precession[3] rates on the magnetic gyrocompass were just unpredictable and unbelievable. And it might just sit there and kind of slowly whirl around the entire face of the compass. So, a big team was necessary just to do the navigation. Sometimes the missions were 16-18 hours long and they would often have two navigators and two rear operators. You had a team of about four people trying to navigate. When you couldn't do celestial, you couldn't see the ground, and you were out of radio contact, you had a very difficult problem."

Having been a navigator, Chris decided to fly on some of these polar missions to see the problems first hand. He first went on missions in the mid-latitude. He went to Europe a few times, and Hawaii, and tried writing down, narratively, what the navigator was doing. This proved impossible. "After a couple hours of that you've had it." When he got back in the office he thought, why not just sample? That's when he developed what's called the sampling technique for activity analysis. Later he found that the British were doing the same thing, calling it Ratio Delay.[4]

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[1]This interview was conducted by Marvin Thordsen, Klein Associates Inc., and John Flach, Wright State University. This account is based on an account written by John Flach.

[2]Because the compass is oriented to the magnetic pole, allowance must be made for the difference in location of the geographic and magnetic poles.

[3]Precession is a turning or tilting of the axis of the gyrocompass due to applied forces. These forces arise from aircraft motions and cause drifting and erroneous indication.

[4]This technique is sometimes referred to as work or occurrence sampling and is discussed in most texts on work design. For example see, Konz, S. (1990). *Work design: Industrial engineering*. Worthington, OH: Publishing Horizons, Inc. Ch. (p. 83 - 100). or Richardson, W.J. & Pape, E.S. (1982). Work sampling. In G. Salvendy (Ed.). *Handbook of Industrial Engineering*. New York: Wiley-Interscience.

Instead of trying to write down everything that the navigators were doing for 16-18 hours, Chris decided to classify their activities at each sampling point, and he sampled every five seconds.

He developed his code by flying missions and just recording what they were doing. If the navigator was working on charts, Chris might jot down "C." Every five seconds he would jot down something. This worked fairly well. Fortunately, he knew exactly what the navigator was doing any time he looked at him.

"Unless you understood the job, you'd have real trouble. Aerial navigation is a very specialized job. It's a pretty professional job involving a lot of calculations and log keeping, and various instruments, and so on. Unless you were pretty familiar with the job, you'd have real difficulty figuring out, at times, what he was doing. Is he drawing a line or plotting position? Is he about to take celestial shots and doing a little pre-computing, or what?"

Chris' categories included map reading, celestial shooting, radio work, E6B computer work, among others. There might have been a dozen categories, including rest and eating. Afterwards, he realized that one could use a sampling technique and observe an entire team. "Suppose there was a team of three. The first five seconds Chris would observe what Operator 1 does. The second five seconds he'd observe Operator 2. The third, Operator 3 and fourth back to Operator 1."

He tried the technique out experimentally back at the lab. He and a colleague, Jim Smithson, went up on an old C-47. Jim had about four or five people he was watching with this interval sampling. It turned out to be a very reliable technique. Later, researchers actually checked the sampling technique against a very detailed analysis taken with a motion picture camera. Reliability was very high—up in the .90s.

With this activity sampling method in hand, Chris was ready to examine how navigators spent their time on these Arctic missions. He started with some control missions, for a baseline condition. The controls were mid-latitude navigation. Chris flew over the Atlantic and the Pacific, finding out what navigators did for those expanses. Then he did the Arctic missions. He was looking forward to seeing the North pole, but he never did. When the airplane he was on flew over the North pole, it was so cloudy, he couldn't see down and he couldn't see the sky, although he did see it on the radar scope.

"In the Arctic, the navigators didn't have a decent radio, so they just kind of felt their way until they could see the stars or see the ground, or at least get drift readings. You get incredible wind shifts in the Arctic, that's the problem. And you get very high velocity winds. You may get winds of 60, 70, or 80 knots, and if you're wrong on the direction, you can end up a long way away from where you think you are."

Chris flew out of Fairbanks, Alaska in three 18-hour missions, and then he flew over to Goose Bay, Labrador. In Goose Bay he got a chance to say hello to Charles Lindbergh. Lindbergh was apparently doing some flying for the Air Force - all by himself; "no crew or anything."

Chris's philosophy was that you had to become a part of the team, do the job, and be accepted as one of the crew. For example, during a pre-flight inspection he would stand at the end of the line of crew to be counted as one of them. When it came time to start the aircraft up, he would get in line to help turning over the propellers. The biggest compliment he could get was to be asked to go out for a beer by one of the crew after a mission. This philosophy was maintained for all his projects, even when he didn't know the job himself, for example working down on the factory floor.

One trip in particular stood out. They were up for 16-18 hours and Chris got hungry first, opened his boxed lunch, and found one of the most unlikely things you can think of to make a sandwich out of: cold pancakes. He had cold pancakes between two slices of bread. He thought, "well, the cook down there knows I'm a civilian, and he's just trying to make fun of me." And he told the pilot, "Gee, a cold pancake sandwich!" It was miserably cold on the airplane. The pilot opened his, and he too had a cold pancake sandwich. "When the airplane hit the ground, it had barely stopped rolling before the pilot was out of that airplane and he was running for that cook shack. He was furious. The poor cook said, look, the supplies didn't come in and the only thing I could think to make sandwiches out of was pancake dough."

Chris worked out a regime of resting about ten minutes out of the hour. He had to last for 16-18 hours. He took samples every five seconds for 50 minutes. And he tried to vary the ten minutes because the navigator's work is quite periodic. If he's a good navigator, every 15 minutes, he might read drift, and every 30 minutes, he might take a sun shot. That's why it was important to stagger the ten minutes.

The data from the Arctic missions were strikingly different from what he found at the lower latitudes. Chris's data showed what the additional burden was on a navigation team. In the mid-latitudes, one navigator was doing the entire job. In the Arctic regions, they had teams of four or five trying to do the navigation. If they had had a decent gyro, one or two guys could easily do it. One of the penalties for the extra crew members was that every crew member you added cost you in terms of fuel. Just adding him and his gear, and his food, and everything else, including all kinds of survival gear all over the airplane, added up to a hefty penalty. An individual crew member at that time was worth about 1200 lbs. of fuel.

Chris had not discovered the need for better directional equipment. Anybody who ever navigated in the Arctic region knew that. The significance of his findings was to lend substance to the need. He provided some quantitative data about what was happening in Arctic navigation.

Back at Wright-Patterson Chris wrote a few reports. He consulted on these reports with friends in the old Equipment Laboratory on base. They were responsible for developing better arctic navigation equipment. The gyros at the time were simply inadequate. They might have precession rates of five degrees an hour, or they might generate a precession rate of forty degrees an hour. The navigator had no way of telling. They all agreed that with better gyroscopes, navigators wouldn't have to rely on their magnetic compasses.

> Eventually the Air Force did put significant amounts of money into the development of gyrocompasses primarily with Minneapolis Honeywell. The end result: the gyrocompasses now are so accurate that you can fly over the pole or anywhere in the world and the precession rates are insignificant. And without the Air Force's work on gyroscopes, the space program would have run into great difficulties.

Summary

It's hard to document the impact of studies like this. The Air Force may have gone ahead even without the data. Possibly the data Chris collected speeded up the program, but no one can say by how much. In an organizational setting with overlapping teams, there is rarely a chance to tease out the influence of any single strand of data.

In this project, Chris learned about the value of field data. He learned the importance of actually going out and trying to experience the job himself or interacting very closely with people who are doing the job, a lesson he carried with him throughou his career.

He also learned that there has to be full disclosure of what the researcher is there for. It doesn't help to flash letters in front of them, saying, "General so-and-so said I could come up here and fly." "It's important to meet with the people being studied, tell them quite candidly why you're there, and what you hope to do. You're not there to check on them, you're there to check on method, and so on. You're not there to try to denigrate anyone's activities, reputation, or anything. You're there to help them, really, in the long run, you hope what you do will help them." Chris also sent to the teams he worked with a copy of the reports he wrote as a result of being there. They seemed to appreciate that. The reports gave full credit to the personnel of the units, and that helped them understand the importance of their contribution.

A further lesson was the importance of trying to set up something consistent with fairly decent experimental methods in the field. That was the rationale for studying the mid-latitude flights, as a control group. You can't set up perfect experiments in the field, but often you can get some kind of control data, some basis for comparison. In this case, the differences between the two latitudes, the high-latitudes and the mid-latitudes, were just striking.

Going Ballistic for Human Factors Standards

An Interview with Donald Topmiller

How does human factors knowledge get incorporated into the design process? How does the human factors engineer add clout to design and redesign recommendations?

These are some of the problems that have frustrated HF experts for years. In the mid-fifties Donald Topmiller of the Aero Medical Research Laboratory was facing the very same questions. He and his colleagues were often required to take part in Developmental Engineering Inspections and provide design recommendations. However, the recommendations they made carried little or no weight; they had no official documentation to back them up, and the contractors were not required to follow human factors guidelines that had been set out in various unofficial publications. The contractors were thus not responsible for the incorporation of human factors recommendations.

The result of these problems was poorly human-factored systems, and it frustrated HF engineers to know that they had many recommendations to offer, but they were still having little impact on design.

In the fall of 1957 Lt Col Bob Lacey, a former Aero Medical lab researcher, invited Topmiller and his three office mates (Lt "Kib" Kibler, Dave Greek, and Charlie Bates) to come to the Ballistic Missile Division's TBST facility at Cape Canaveral. He wanted them to take a look at some of the equipment and systems being implemented at the Ballistic Missile Division and to make some informal evaluations of these new systems. With the increasing intensity in the cold war, the Ballistic Missile Division had been introducing many new systems and equipment with little regard for user requirements.

> The initial catalyst for the impact of this team was the invitation from a former AMRL researcher. The fact that Lt Col Lacey had worked at AMRL meant that he was aware of the work that went on there and of the potential impact of that work on his new assignment to Ballistic Missile Division and the problems he was seeing.

As an example, the refueling personnel at Canaveral were required to wear protective gloves which physically prohibited them from doing their job safely. The cumbersome protective gloves meant that they were unable to undo the fasteners on some of the highly flammable and toxic liquid containers. Operators were removing their gloves in order to do the job and in doing so exposing themselves to great hazards. In one instance, rocket fuel had soaked into a refueler's undergarment and was ignited in the locker room when the man lit a cigarette.

A further example, discovered by Topmiller and his associates, was the fact that control consoles for a particular system were being designed by six different subcontractors none of whom had any regulations guiding the human factors input for their designs. It was conceivable that the six different contractors could have come up with six different ways to accomplish the same function.

Topmiller and his colleagues spent over six weeks at Cape Canaveral. They soon began to wonder at the inconsistency in the application of human factors knowledge in the design of many critical systems and important equipment. At the end of each day the group would swap "horror stories" from their day's evaluation. They began to talk about what might be done to improve the problems at Cape Canaveral as well as improve their chances of having an impact on system development at an early stage. They were tired of being brought in when too much had already been invested in the design, "after the tin had been bent."

The opportunity to bounce ideas off one another as office-mates and during the trip to Canaveral fostered teamwork, brainstorming, and communication on a daily basis.

On their return to the Aerospace Medical lab at Wright-Patterson, Topmiller and his colleagues submitted an internal report on their trip to Cape Canaveral describing what they had found. They also began to throw ideas around their office about how they might accomplish the fix.

Part of the "fix" for Cape Canaveral's problems was the same fix needed by the human factors engineers to add clout to their design recommendations: mandate certain human factors practices and standards, and require designers to document their human factoring efforts to demonstrate how these efforts complied with the required standards.

Up until that point the process of including human factors in design had relied on guidelines like the Joint Services' Human Engineering Guide to Equipment Design. However, these guidelines were not required to be followed and provided no ammunition for the human factors engineer to use with the designers who had done a poor job of considering the needs of the user.

Having appreciated the existing problem, Topmiller and his colleagues decided to invest some time into doing something about it rather than just sitting back and complaining about it.

In order to standardize the lab's evaluation process, Topmiller, Kibler, Bates, and Greek put together a checklist of human factors considerations for design. This informal checklist allowed them to participate in an evaluation with a means of providing standardized feedback to the designer based on their checklist. They were now able to point out improvements to the design and point to the evidence to support their suggestions.

In 1958, following the internal checklist, the group put together a technical document, MIL-H-26207, which listed requirements for contractors to provide certain types of information recording the incorporation of human factors guidelines into the design process. These requirements included the need for information about any functions analyses, task analyses, and anthropometric analyses that had been used to help the design. If a design failed to take into account certain types of human factors information, the human factors engineer now had a document to point to support the recommendations. This data specification also encouraged a standardized approach to the incorporation of human factors guidelines into design.

The team continued to pursue the idea of regulation and standardization by producing the first military standard document which provided human factors standards and requirements for design, at the request of Lt Col Lacey. MIL-STD-803 laid out human factors standards to be achieved by

designers of ballistic missile systems. This was soon followed by a similar document for aircraft systems design and ground equipment design (see, *The Case of the Uncommon Consoles: An Interview with Steve Heckart*).

One of the surprising aspects of the story is the time scale. Within a period of less than three years, Topmiller and associates had produced and published standards for the Ballistic Missile Division. This rapid time course is unlikely to occur that quickly today.

These were the first steps to requiring systems designers to think about the operators of their systems and how they might design more effectively for the user. Not only that, but these standards provided the means by which the HF engineer could evaluate designs based on accepted standards, and provide redesign recommendations with documented support. These formalized human factors specifications and standards launched the field into an era of acceptance by the engineering community in government as well as in industry.

The Case of the Uncommon Consoles

An Interview with Steve Heckart

The balance between centralization and decentralization is difficult at all levels of organizations. If you centralize too much, you need all sorts of special exceptions and these keep growing until the system bogs down. If you centralize too little, you have waste and duplication and inefficiency. There are no simple answers, and the only guarantee is that the issue will be a constant challenge.

One way that the issue of centralization arises in the field of human factors is over standards. In the previous case we found Don Topmiller trying to impose standards for the Ballistic Missile Division. This case describes how Steve Heckart took those standards a step further, to form MIL-STD-1472, the central directive for the use of human factors in design. Military standards are now in place for the combined services; Steve Heckart served as the focal point for Air Force inputs into the standards.

Prior to 1958, there were no human factors standards for designers and contractors to adhere to. In the late 1950s Aero Medical Research lab was instrumental in getting the first standards mandated, requiring contractors to provide evidence of some consideration of the human operator in systems design (see *Going Ballistic for Human Factors Standards: An Interview with Don Topmiller*).

Heckart's particular interest was in standards for the design of the ground-based equipment and systems, maintenance equipment, and control panel design. When Lt Col Bob Lacey had invited the Aero Medical laboratory people out to Ballistic Missile Division at Cape Canaveral to talk about developing a human engineering design standard, Heckart and his co-workers had also been approached by Capt Ed Milauckas on the same topic. Milauckas was concerned about the design of missile complexes, launch officer consoles, and silo consoles and equipment.

Each specific system had its own custom-built console. The contractors loved it because they had a brand new design problem for each new project. Milauckas hated it because the Air Force wound up spending large sums of money each time, and because the differences between consoles made the operators' jobs more difficult, and made it harder to maintain all the consoles. The differences seemed trivial and arbitrary to Milauckas and he felt that it should be possible to develop a common format for everyone to use.

> Heckart's goal was that all the lab's research should have some application to the mission of the Air Force, and not just for the purposes of scientific publications. One way of achieving this was to provide data which could be applied as design criteria in standards.

Milauckas and Heckart discussed the idea that if a standard set of control panels could be designed according to human factors design criteria, the cost of producing these standard panels would be less than having custom built panels for each application. Not only that but the equipment would all be standardized and designed according to the human factors standards. The panels could be made so that they could be adapted for special purpose systems, but the added cost of adaptation would be less than having the contractor create new designs for each system.

Charlie Bates, Ken Kennedy, Milt Alexander and Heckart kicked around the question of whether standard control panel design could be accomplished, and whether expensive special purpose

designs were needed or could be adapted from the standard panels. Up to that time, human factors and ergonomics had primarily addressed hardware issues such as the shape of knobs and buttons, the motions and sensitivity to operate tools while wearing gloves, the reach distance for instruments in a cockpit, and so forth. Applying human factors standards to the logic of console design was something new. Yet it seemed feasible. The team agreed that they should take a shot at it.

> Heckart's criterion for evaluating whether he had done a good job was to see how many pieces of tape, grease pencil marks, job aids, and other work arounds had been applied to a piece of equipment. If there were too many then the designers had done a poor job and the design standards had failed, or had not been applied.

Heckart knew that the data for these purposes already existed, it was just a matter of finding them, applying them to the control panels, and testing their suitability for the tasks that the operators would have to accomplish with them.

Heckart and Ken Kennedy, the reach data specialist, began to draw the relevant data together. Heckart observed the operators who would be using the panels in order to visualize how the control panels were being used. Kennedy built mock-ups of the panels in his shop. After evaluating the mock ups, the standards for several control panel designs were put into a draft standard and eventually were implemented in the military standards document, MIL-STD-803A. The time course of the whole project was only 18 months from Milauckas's problem statement to the first version of MIL-STD-803A.

> Heckart believes that the human factors standards required an advocate in the design office who could make those tailoring decisions and apply them to the design contract. This person should be administratively responsible to some external organization, such as the Aero Medical lab, and not to the design engineers in the SPO. The Army did a better job than the Air Force in this respect, as their human factors people were spread throughout the arsenals and were in a better position to advocate the inclusion of design standards in contractors' design specifications and also to ensure the appropriate tailoring of standard design criteria to fit the specific application.

One of the recurrent problems is that once standards have been provided to the design community they don't get implemented into design contract requirements. One of the reasons they aren't put into the design contracts is that the standards are so inclusive that some sections may not seem to apply to the particular design. The primary purpose of the standard control panel designs was to be adaptable for tailoring to the needs of specific users. Second was the goal of cutting production costs of custom control panel designs for every new project. The Army has made even greater use of MIL-STD-1472 than the Air Force, because the Army provides advocates and interpreters of the standards in the design offices.

"Measure Me Workload!"

An Interview with Bob O'Donnell

In 1977 Charlie Bates made Dr. Robert O'Donnell head of a new branch and told him to go out and "Measure me workload!" The Workload and Ergonomics branch was thus born. O'Donnell set about building the branch and establishing a reputation for its work. At the time workload was not the buzzword that it later became. In that sense, Armstrong lab was leading the way again.

The fact that no one knew what workload really was did not deter O'Donnell from accepting the challenge, but O'Donnell is now the first to admit that he is still unable to measure the elusive concept of workload. Yet, the research that came from that branch under O'Donnell's leadership has produced some ground breaking work in the measurement of human performance and in the assessment of operator readiness for duty.

O'Donnell knew the research approach that he was most interested in pursuing. And he also knew the research approach that people thought would have the highest likelihood of success. But given the importance he placed on needing a rigorous and balanced program, he added a third approach that was neither interesting to him nor likely to contribute much of use. And it is that third approach, the ugly duckling of the program, that has had the greatest impact.

An Integrated Approach

With the challenge issued by Bates, O'Donnell set about organizing his branch. His first concern was to ascertain what it meant to measure workload. There was not a wealth of literature out there addressing the issue. The literature that did exist could not provide a succinct definition that would enable O'Donnell to produce an instrument that would give him a single reading of workload.

O'Donnell made his first key decision: he was not going to find workload in any one place so he was going to need an integrated approach. He divided the branch into three specialty areas: physiological assessment, subjective reporting techniques, and behavioral and cognitive performance assessment. He added in this third component because it seemed to balance the program. He didn't have any expectation that the work on the behavioral/cognitive measures would be successful, but he thought it would help to focus the other two approaches. Nevertheless, to make sure his biases didn't get in the way, O'Donnell treated each approach as equally valid and worthy of equal attention, until one proved its worth over the others.

> O'Donnell's own background was in physiological performance assessment, and he could have been tempted to stick with what he knew and put all his money on one horse, the physiological measures. Instead he chose a balanced approach bringing in experts from other disciplines, and favored no one line of research.

In order to develop these three approaches for an integrated view, O'Donnell knew he'd need an eclectic group of researchers who were self-directed and focused. His next task was to identify those individuals and bring them on board.

Clark Shingledecker, an NIH Fellow and SRL employee, was taken on to handle the behavioral assessment group, working with Mark Crabtree. Gary Reid, who was working in personnel

testing at Williams AFB at the time, was recruited to handle the subjective workload assessment issues. The third group, the physiological group, was headed up by Glenn Wilson after a chance meeting with O'Donnell when Wilson brought his class from Wittenberg University to look round the laboratories. A fourth figure was brought on as an information resource—Tom Eggemeier from Wright State University (now the Chair of the Psychology Department at the University of Dayton).

Playing the Circuit

Having mustered his troops, O'Donnell let them do what they did best. His only mandate was that the group should be a high-profile group in meetings and conferences, and that, even though they were working in their own groups, they would always try to present their work in integrated forums. The workload branch became known for stacking a session and presenting their work as a team. O'Donnell would introduce the integrated approach, Eggemeier would provide the background information from the literature, Reid would go over the subjective work, Shingledecker the performance work, and Wilson the physiological work. The well-greased workload presentation machine became a high visibility laboratory with a respected reputation.

O'Donnell intentionally pushed the high profile of the branch, the purpose being to invite criticism. The workload group was as much searching in the dark for the elusive concept as all the other researchers interested in workload. By taking a spotlight, their integrated approach was open to feedback, and not all of it was supportive of their work.

The Ugly Duckling

Shingledecker's work in the lab had produced a cognitive performance test battery called the Criterion Test Set (CTS). Examples of the tests used include tracking tasks, mental rotation, continuous memory, semantic reasoning, and the Sternberg reaction time task. Shingledecker had originally seen the purpose of the test battery being to provide performance criteria against which other measures of workload could be tested.

However, the CTS was never actually used as it had been intended. Instead, people realized that it could be used to measure how much environmental stressors degraded cognitive performance. If we expect pilots to function well under extreme conditions, we should at least try to find out what these conditions do to the ability to think clearly. The CTS allowed such studies to be done. Different degrees of a stressor (e.g., thermal or chemical) were applied while the subjects took the CTS tests. The scores from the CTS were compared to assess how performance was degraded by the stressor. This unintended use meant that the CTS battery could provide answers about how stressors affect performance.

The next step occurred in 1980, when Fred Hegge of the Army approached O'Donnell who was part of a Tri-Service commission on counteracting chemical weapons. The military had identified a number of agents that worked to counteract the effect of chemical weapons, but no one was sure how these agents would affect the pilots themselves, in terms of the cognitive and behavioral performance. Would they be less likely to make discriminations, or form judgments, or respond quickly? The Air Force was more advanced in the area of stressor effects on performance than the other services, so O'Donnell was given the task of developing a technique to assess the cognitive performance effects caused by these agents.

O'Donnell's group adapted the existing CTS which had already proven helpful in the area of assessing stressor effects. In 1983, they produced a new cognitive assessment battery which utilized about 60% of the tests from the CTS, and added in several other accepted and established cognitive performance tests to make up the complete test battery.

This cognitive assessment battery is still being used today, and has also been adapted by NATO as a standard performance test battery for cognitive performance assessment.

O'Donnell retired from the Armstrong Laboratory in 1985. Since then he has further modified the cognitive assessment battery to provide an easy to apply, easy to use, cognitive performance assessment tool for workplaces demanding high levels of alertness and accurate judgments. Workers take a 3-6 minute test before they go on their shift. If they fail to reach a criterion level based on their baseline scores, they are considered unfit to perform their shift. This test battery has been backed by the FAA, Amtrak, Metro Machine Shipping Company, and the New York Metro as a quick assessment of mental alertness before a working shift.

Although O'Donnell was unable to satisfactorily grant Bates' original request, much has been accomplished by the branch. The researchers have made important findings in the area of physiological measurement techniques, and the development of an effective subjective workload assessment technique (SWAT). What was not expected was that the most successful applications have come from the ugly duckling, cognitive performance assessment techniques.

Moments of Inertia

An Interview with Charlie Clauser

How much does your head weigh? How does the center of mass of your body shift when you move your limbs? You may not know the answers to these questions, but Charlie Clauser does. This is a story of baboons and body parts and an unplanned journey that resulted in a set of data from 1974 that is still being used to produce manikins for crash testing, restraint testing, ejection seat testing, and similar equipment design issues. The data are even being used to explore human performance, such as in gymnastics, using computer modelling techniques. But it all happened by accident.

The Field of Physical Anthropology

Physical anthropologists trace the size and shape of human structures such as bones and skulls. Academic physical anthropologists study basic issues concerning the origin of the species, the history of man. Applied physical anthropologists try to use data describing the human form, its dimensions, characteristics and properties, in order to model human and human-machine performance.

When Charlie Clauser completed graduate school at Indiana University in Anthropology in 1956, he joined the applied camp, going to work at the Aero Medical Research Laboratory at WPAFB. At that time, the Anthropology group was investigating the relevant dimensions and characteristics of the human body in order to help with cockpit design, ejection seat design, and the design of other close fitting personal protective equipment. (Today this group is called the Computerized Anthropometric Research and Design Group of the Design Technology Branch. The term "anthropology" reminded too many people of Margaret Mead in Samoa, and was dropped.)

Free-Floating Curiosity

In the late 1950s, the Air Force began to get heavily involved in the space program. NASA had not yet been formed. One topic of study was the problem of extra-vehicular activities (EVA). Astronauts were going to have to use self-maneuvering units to perform tasks in zero-G environments outside the spacecraft. As a result, the anthropometry group at AMRL had begun to wonder how movements of the human would affect the stability of the maneuvering system. In order to build a predictive model, the scientists needed to understand the mass distribution characteristics of parts of the body. Once the anthropologists at AMRL obtained those data, they could help system designers predict the flight characteristics of astronauts. If the Astronaut Maneuvering Systems was discovered to be unsafe or unstable in any way, they could redesign it based on the inertial properties of the astronaut from the work of AMRL anthropologists.

Soon, data sponsored by AMRL began to appear. The North American study of mass and centers of mass of the living human body was published in 1963. In 1964 Hanavan published the first model of the inertial properties of the full human body based on these data. Clauser had initiated and monitored these projects, but he was still curious about how the data on mass, and center of mass, would affect Extra Vehicular Activity. During this time, Clauser's desk was often strewn with wooden "limbs," which he used to try to simulate the body's reactions to various movements imposed by a vehicle in space.

A Lucky Encounter

Clauser had been collaborating with a group from CAMI (Civil Aero Medical Institute) run by the FAA in Oklahoma City. They had conducted a cadaver study, sponsored by NASA, in order to characterize the mass, location of the center of mass, and volume of the human body. This had been published in an AMRL tech report in 1969.

In 1972 Clauser attended a meeting on the west coast and prior to returning to Ohio he arranged to visit an old graduate school friend, Joseph Young, with whom he'd worked at CAMI on the first cadaver study. While he was there, Dick Chandler, who did research at CAMI, had seen some of the work that had come out of AMRL and the first study, and took the opportunity to talk with Clauser. Thus far, AMRL had concentrated on studying mass, center of mass location, and volume. Chandler wanted to know the moments of inertia of the body segments.

Clauser's response was "What's a moment of inertia?" Not being an engineer, he wasn't familiar with the idea but he quickly realized its importance once Chandler had explained what he meant. He realized that the stability of a small space sled would be governed by the inertial characteristics of the man/machine system and that these characteristics would change with movements of the astronaut's arms and legs and head. The mathematics and physics of these body movements were the key to the puzzles that he had been struggling with in order to understand how a body would move in a zero-G environment.

Equally important, Chandler knew of a way to study these inertial forces. One of his researchers at CAMI, Dr. Herbert "Mack" Reynolds, had just completed such a study with baboons. Therefore, the techniques for making the necessary measurements had already been developed. Unfortunately, CAMI did not have the manpower to undertake such a project for the human body. Chandler asked Clauser and his colleagues if they would be interested in conducting the research using CAMI's facilities and AMRL's manpower. CAMI also had another important resource, which was their very well-equipped anatomy laboratory; CAMI also had access to one of the best cadaver populations in the country.

> Coincidental circumstances led to the formation of this collaborative project. Clauser dropping in on his friend; Chandler offering Clauser an opportunity to work again in a well- equipped lab on a mutually interesting topic, the inertial properties of the human body; and Reynolds having experience in the measurement techniques of these properties from his baboon study.

Cross-Country Collaboration

Clauser returned to Dayton and wrote up a short proposal to initiate money from the National Highway Traffic Safety Administration (NHTSA). He proposed the research that had been discussed at CAMI and tied it in to applications to transport safety and the self-maneuvering units being developed by NASA for EVA. The data from this line of research would also have applications in the design of seat restraints in vehicles and in the design of the crash test dummies used to evaluate vehicle safety.

> The applied anthropology community fostered collaboration and cooperation between researchers and research institutions. This enabled Clauser to conduct work using the manpower of the Armstrong Laboratory, in a well-equipped anatomy laboratory owned by the FAA, with researchers with various areas of knowledge from the field of applied anthropology, and the combined funding from several government agencies: NASA, NHTSA, FAA, and AMRL.

The funding was approved and Clauser had the go-ahead for the study. In 1973 Clauser, Reynolds, Young, and Chandler began the somewhat macabre task of measuring the moments of inertia of the body parts of six cadavers in the CAMI anatomy lab in Oklahoma City. The research project had been suggested, formally proposed, funded, and started within the space of one year, a remarkable achievement in itself. The measurement procedure itself included encasing each body part in a box, attaching it to a pendulum and setting the apparatus in motion. Measurements were taken for the weight, center of mass location, and principle moments of inertia of the six cadavers. The segments were also measured individually for mass, center of mass, moments of inertia, and volume.

One of the aspects of this opportunity presented to Clauser and his colleagues was the fact that he was allowed to pursue the project in the first place. Clauser commented that, in those days, a researcher worked in an environment which encouraged the pursuit of questions that he or she found interesting, even if the original idea did not come directly from the lab, or if the work itself was not to be conducted in the lab. The positive environment in the Armstrong lab supported and encouraged scientific curiosity and creativity in research related to the lab's mission.

The data collected in this study were reported in an AMRL technical report (AMRL-TR-74-137, Chandler, Clauser, McConville, Reynolds, & Young, 1975). The data are still used today as the standard for modelling mass distribution characteristics including moments of inertia for the human body. Ironically, the most immediate use that the Air Force made of these data was for designing ejection seats, rather than for Extra Vehicular Activities. In order to predict the ejection characteristics of the pilot in the ejection seat, design engineers need to know not only the weight distribution and "flight" characteristics of the seat, but also those of the pilot. The data collected by Chandler et al. from both the CAMI cadaver studies directly addressed this issue.

The work from these types of studies tends to last for significant periods of time. Some of the earliest anthropometric data gathered in the 1800s are still referred to. Clauser and colleagues' data from these studies are maintained as the standard of the day and may well be so for years to come, providing accurate data for manikins and performance modeling.

General Reference

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Crossing the Barrier: Developing SAINT to Model Human and Equipment Dynamics

An Interview with Jerry Chubb

Design engineers rely on simulations to calculate a variety of factors and play what-if games to examine different contingencies. But in the early 1970s, design engineers did not have a way to include human performance issues in their simulations. In fact, they were suspicious of human performance data and uninterested in the attempts to model these data. There was an invisible barrier between the design engineers and the human factors community. Jerry Chubb was in charge of a project to help design engineers use human performance data. He had to find a way through this barrier. The software simulation he and his team produced, SAINT (Systems Analysis of Integrated Networks of Tasks), accomplished the trick. It took about six years to develop, from 1970 to 1976.

Jerry started working for the Human Engineering Division in 1963. He had a M.S. in psychology from Ohio State University. (He later went on to get his Ph.D.) He inherited this project without much warning. The original stimulus for the effort was the concern in the late 1960s about how nuclear attacks would affect weapons systems. The Special Weapons Center at Albuquerque, NM commissioned Vulnerability/Survivability studies on airplanes such as the F-106, the B-52, and others to see if the equipment would survive various weapons effects—the thermal pulse, the burst effect, transient electronics effects, electromagnetic pulses, and other ionizing radiation impacts. But there was one component of the weapons system that had not been well studied—the pilot. Animal research had been going on, but it was hard to extrapolate from these experiments to the psychomotor and cognitive processes of pilots. This gap was worrisome. Contractors studying this problem had simply cited the results of radiobiology research studies. Little was said about possible performance impacts.

As a result, a meeting was held between representatives of the Air Force Weapons Laboratory and Don Topmiller and Bob O'Donnell of the Aerospace Medical Research Laboratory. Topmiller and O'Donnell suggested that the problem could be solved using modelling techniques such as the Siegel-Wolf method. This was a simulation model that had been developed by the Navy in the early 1960s to use task analysis data to estimate task times, and had evolved into a technique for comparing activity timing patterns under different conditions. You could use the Siegel-Wolf simulation to compare task timing patterns before and after ionizing radiation, in order to estimate the magnitude of the performance decrement. Therefore, the solution seemed close at hand. As a result, the Air Force Weapons Laboratory agreed to fund the project. The Human Engineering Division never put any funds into the project.

Topmiller and O'Donnell were pleased to have closed the deal, and considered their job done. Jerry Chubb was told that he was in charge of the new project. This was in 1970.

Chubb quickly found that the task was not as simple as he had been led to believe. He began by arranging to have the Siegel-Wolf technique modified so he could use it for the Vulnerability/Survivability program for the F-106, an interceptor aircraft. When Siegel did a literature review of the radiobiology experiments, he discovered lots of holes that limited the generalizability of the data. If they wanted to estimate human performance degradation, it was clear that they would need to rely on interpretations of the radiobiology data by Subject-Matter Experts,

radiobiologists, to generate subjective estimates of how behavioral and cognitive performance would degrade as a function of different radiation dosages and at different times following exposure.

The subjective estimates of performance degradation were based on animal studies and case studies of humans exposed to radiation. And to get these subjective estimates, he needed to derive cognitive scales. He used Guilford's Structure of Intellect model, selecting three of the five scales (divergent thinking, convergent thinking, and cognitive/perceptual processing). He added in some psychomotor factors that would be relevant to flying. The subjective estimates provided him with a set of curves showing how for different dosages the performance degradation would increase sharply following exposure, and then the pilot would gradually recover. He linked these curves to other estimations of the probability of success for a mission. In order to make sense of all these data, a simulation model was needed. The simulation served as a bookkeeper to keep all the branching and permutations straight.

In 1971 Jerry led a research contract to extend the work to the B-52, which had a larger crew than the F-106. This contract developed a model that could handle a ten-person crew. The model also incorporated other psychological constructs that would be important for describing the performance of multi-person crews.

In 1972, Jerry went shopping. He had found that engineers he discussed the project with were reacting negatively to the Siegel-Wolf technique. As far as he could tell, the negativity was due to a few important issues:

- The Siegel-Wolf technique had been developed by behavioral scientists for other behavioral scientists, so engineers didn't feel that it was speaking to them in their language.
- The Siegel-Wolf technique was based on psychological data rather than the hard data engineers preferred.
- Chubb's version of the technique was based on Subject-Matter Expert estimates, not even empirical data.
- Chubb was relying on the Guilford model, which seemed alien to the engineers.
- The basic Not-Invented-Here syndrome generated suspicion of any tool coming from outside the engineering community.
- Possibly the strongest negative was that the Siegel-Wolf technique only simulated the human performance, and didn't cover the performance of the equipment being operated.

If he was to cross the barrier and develop a tool that could be used by engineers, Jerry had to counter these criticisms. He would have to abandon the Siegel-Wolf technique and find a replacement.

The shopping trip led him to examine existing engineering simulations that he might use and adapt for his purposes. Jerry attended the 1971 Winter Simulation conference, going to tutorials on various simulation languages. He considered a few of the leading simulations but found them inflexible. They required special purpose compilers and would be expensive and labor-intensive to adapt. But in one of the tutorial sessions he heard about a simulation that mimicked the more powerful programs. It was FORTRAN-based, and would be practical to modify for the needs of

his project. It had been developed by Dr. Alan Pritsker, an engineer who had taken a position at Purdue University. Jerry drove to Purdue and met with Pritsker.

They discussed the fact that Pritsker's methods were aimed at a linear, time-invariant network: the features of equipment that could be easily represented in a special adaptation of the simulation language Pritsker had developed. However, Jerry needed to work with a nonlinear, time-varying network, namely, the task operators performed using the equipment. This was a radical idea for the time, since psychologists were using an information processing model of human performance that was basically a linear model. Engineers had not yet addressed many problems that were nonlinear and time varying. Analysis of these problems was difficult, so engineers often looked at a series of time invariant linear models to approximate the more complex analyses.

Pritsker was intrigued. If they succeeded, this kind of simulation analysis technique would serve the needs of both the engineering and psychology communities. Pritsker soon received a research contract, with Jerry as contract monitor, and went about implementing the Siegel-Wolf model within his engineering simulation package. In developing SAINT (Systems Analysis of Integrated Networks of Tasks), Pritsker generalized the Siegel-Wolf model substantially.

SAINT, the new simulation, was used to represent the vehicle dynamics and the human operator activity for the B-52 refueling task. This involved two moving vehicles (the B-52 and the KC-135 tanker). The tanker tried to fly straight and level. The B-52 pilot pushed the control yoke and throttle. The simulation captured the dynamics of the system and the activities of the pilot and aircraft. No one had ever before connected a network simulation with a continuous process modelling. The solution was to link continuous processes with states. For example, altitude is a state variable. A B-52 can go up and down relative to the tanker. At any point in time where the altitude crosses a line (e.g., gets too close to the tanker) this is called an event. The event will trigger a need to reduce power and to reduce altitude, which then changes the state variable. In this way, the simulation captures the regulation of a continuous process (refueling) by a discrete task. The outcome is man-machine modelling. The simulation places a monitor on the continuous state variable, to produce events. It places regulators on tasks to change state variables (e.g., changing the throttle from 75% to 50%). This concept then applies to other regulators and state variables in a similar fashion.

The next step was to embed into SAINT a Fortran sub-program to moderate the task time as a function of other factors, such as stress, or anything else. These were the psychological levers that allowed the simulation to be used to calculate how different conditions, such as exposure to radioactivity, would affect performance measures such as task time, which in turn affected probability of success. This last step was under the direction of Deborah Seifert and completed the initial version of SAINT, as summarized in a 1978 technical report by Seifert and Chubb.

To test the new simulation, the research team entered in a set of data on Remotely Piloted Vehicles. SAINT worked very well. The rigor demanded by a simulation even enabled them to discover some areas where the behavioral studies had not been done quite right. In addition, they were able to use SAINT to show equipment effects and what-ifs that were reasonable conjectures that had not been previously explored.

SAINT has been successfully applied to workload estimations during design of the Precision Location Strike System. One contractor used SAINT to model the B-1 radar and establish if a multimode radar could keep up with the information processing demands of the task, a strictly engineering analysis application of SAINT. It has also been used to look at message traffic and queuing delays. It was used on a Navy P-3 upgrade project to look at operator workload; SAINT

verified the feasibility of the operator activities. SAINT has been adopted as a working tool by governmental organizations in other countries as well (Canada, the Netherlands, Germany). It helped to influence the development of other simulations, such as SLAM (Simulation Language for Alternative Modelling), a program that is still being marketed. And it has given birth to MicroSAINT, which is written in C rather than Fortran. MicroSAINT has been used in projects such as examining the effects of performance degradation from nuclear, biological, and chemical attacks.

SAINT led the way in demonstrating the feasibility of a simulation that can work through the consequences of activity sequences, and can trace the way individual errors can affect the timing and pacing of an operator's activity, especially in conjunction with other operators.

The Case of the Wavy Windscreens

An Interview with H. Lee Task

Experts see different worlds than the rest of us. Where we may painstakingly try to figure out how to handle a difficult task, they can take one look and know how it ought to be done. When we have a chance to watch this ability in action, we can get a richer sense of what expertise looks like.

Lee Task is an expert in optics. He received his master's degree in solid state physics before he came to the Human Engineering Division in 1971, and a few years later went back for his Ph.D. in optical science.

The Distorted Windscreens

In 1978 Major Robert Eggleston, Task's branch chief, came to him with a problem to solve: the optical characteristics of the F-16 windscreen were causing pilots to aim incorrectly and miss their targets. When the manufacturers of aircraft windscreens had shifted from thin glass windscreens to thick plastic ones, pilots began to complain of distortions, haze, and multiple images created by the new thicker windscreens. These complaints had been going on for years. The problem of seeing multiple images was perhaps the most frustrating problem. Which target do I aim at? Which set of landing lights should I land on?

> The F-16 SPO approached the Human Engineering Division with this problem because the division had helped out on an earlier problem with the F-111 windscreen. That problem had first been studied by a team of optometrists who had treated the windscreens as if they were like eyeglass lenses. Lee Task had shown that the crux of the problem was in the refractive and displacement properties of the windscreens and that the best approach was based on the perspective of geometrical optics. Eventually, the manufacturers learned to improve their methods to the point where the complaints quieted down, even though the distortions were still a nuisance.

But the F-16 problem could not be ignored. Because no one had found a way to manufacture distortion-free windscreens, the manufacturers had figured out a work-around. They were able to measure the unique distortions in each windscreen, and use these measurements to feed corrections into the Heads-Up Display (HUD) to compensate for the discrepancies.

Now the problem had shifted. While this measurement strategy worked, it seemed very labor intensive. The method used at the time for evaluating the optical aiming error of the windshield required two man-days of effort. A laser was directed through the windscreen onto a wall 100 feet away where a technician would record the movement of the laser shot. This method was replicated for 60 data points. Due to its manual nature the method was prone to error at many stages of the data collection process.

When Task heard about how these measurements were being made he could not believe the inefficiency of the method. "Why are they doing that?" he wondered. He immediately knew there was a more efficient way of collecting the windscreen data using more up to date techniques and equipment.

Task's idea incorporated placing the windscreen on a movement table which would rotate the windscreen through known, accurate increments. A laser (later replaced by an incandescent light source) was projected through the windscreen to a recording device just two feet from the transparency. This device allowed over a thousand data points to be collected in an hour, providing the curve fit data from four different "eye-points:" the HUD position, the left and right eye-positions of the pilot, and the gun camera viewpoint.

Task built the first prototype of the measurement device in the basement of one of the Armstrong Laboratory buildings at WPAFB and the SPO had the windscreen manufacturers ship all their windscreens to the basement for measurement until they could get hold of their own measurement devices.

> The estimated improvements from this system include drastic reductions in man-hours spent taking measurements. There was also an estimated 15-20% improvement in data accuracy from using 1000 data points versus 60 making up the curve, and a more accurate automated windshield rotation device, as well as four eye-point measures instead of just one.

Task patented the system with several co-workers. In 1979, they formed a company and began to sell the measurement devices to the windshield manufacturers who were unable to handle the electronics requirements to make their own devices. The endeavor led to sales all over the world. The endeavor was a richer learning experience and success than a financial one as the market was soon saturated. There's not too much call for aircraft windscreen measurement devices. But the benefits to the Air Force pilots have been immeasurable.

The Pilots who had to be Kept in the Dark

An Interview with H. Lee Task

Landing an aircraft in the dark has never been easy. Landing an aircraft in the dark on a makeshift runway using night vision goggles (NVGs) was a real headache. This was a problem handed to Task and his colleagues by Military Airlift Command (MAC) in early 1982.

The problem was that the transport pilots operating on austere runways were having problems judging the glide slope while landing on these temporary, covert landing strips. (These were often landing strips in hostile territory.) The transport pilots would come in for landing wearing NVGs which reduced their ability to judge their angle of descent relative to the landing strip. This had already resulted in at least one C-130 crash.

Task was acknowledged as one of the experts at WPAFB in the use of NVGs. This was simply because he was one of the first people at WPAFB to have a pair. He had been given a set of PVS-5 NVGs in the late 70s by a friend, an Army officer who had been a former colleague at the Human Engineering Division. Task had optically characterized the equipment following which the goggles had sat on a shelf gathering dust.

MAC had learned that Task and a colleague, Lee Griffin, were knowledgeable about NVGs, and had sought their help on earlier occasions. Once, MAC had needed compatible electroluminescent lighting for covert austere runway landing strip lights. Another time they were having problems with NVGs and the in-cockpit lighting. Task and Griffin had solved their problems and so now Task was the person they turned to after the C-130 crash.

MAC wanted Task to come up with a device which could indicate whether the pilot was on the glide-slope, too high or too low, while he was wearing NVGs. All this had to be done in a way that didn't compromise the position of the special operations ground force.
Task realized that the problem was solvable. He merely had to specify some of the parameters in order to work out the solution. He identified the problem as having three key features:

- He would need a light that was bright enough to be seen through NVGs at long distances without giving away the ground position.
- He would also need to be able to indicate the optimum glide-slope and whether the pilot was deviating above or below that glide-slope angle.
- Finally, the whole system would need to be easily and cheaply made, small enough to be carried by the ground troops, and quickly and accurately assembled.

The first problem he addressed using the old NVGs which were still sitting on his shelf. One cold night he and his colleagues fixed up some old flashing car tail lights at one end of an inactive taxiway on Area B. They drove about a mile to the far end of the taxiway to see if they could see the lights through the NVGs. (While they were driving Task decided to switch off his own car

lights and drive using the NVGs just to see what it was like. Within seconds they were pulled over by the base security police! Task explained that they were testing the NVGs and the policeman backed off, satisfied with Task's response.)

The flashing lights at the far end of the base were clearly visible. The best blink rate seemed to be at a rate of about one flash every two seconds (0.5 Hz). To keep the lights from being seen by unwanted eyes they had housed them in a box with a single aperture and an infrared filter so that only an approaching pilot could see them.

The next problem was to establish how the lights would indicate a glide slope. One set of lights would be placed on each side of the runway. Task set them up so that the system could be set to several different glide slopes for different situations. At the correct glide slope the pilot would see one flashing light and one static light (one on each side of the runway). If the pilot deviated above the glide slope he'd see two flashing lights, and if he deviated below the glide slope he'd see two steady lights.

The final problem was overcome by producing a unit that fit into a briefcase-sized carrying case. The materials used for the system could all be easily obtained. And the design made it easy to set up the system. The lights could be aligned flat to the ground using a bubble level, and different glide-slopes could be set using wedges cut to the correct angle and aligned on the light units.

The bread board units were initially flight tested in April of 1982 to see if the pilots could acquire the indicators through NVGs at sufficient distances, to see if the power was sufficient, and to assess whether the system could be spotted by ground troops who might compromise the position.

In July 1982 the prototype units were officially and successfully flight tested at Pope AFB. (During the flight test, Task noticed something else that was unusual about the way the pilots were landing, but that's another story. See The Pilots who had to Play it by Ear—An Interview with H. Lee Task.) The special operations liaisons asked for twelve units for operational use immediately. So the project transferred from a problem statement to an application in operational use within six months.

The acquisition of twelve units for immediate use by special forces was accomplished through unconventional channels due the nature of the "black" project for covert operations. The twelve units were produced under the official title of Operational Test and Evaluation units. Had the official procurement route been taken it may have taken several years to get this relatively small project in place. Not everyone was happy with this rapid turnaround. MAC was written up by the Inspector General for not following the correct procurement channels!

Although no formal evaluation of the project has been conducted, it can be assumed that these simple devices have improved the safety of the transport pilot landings and guaranteed the safe delivery of important supplies to the ground forces relying on them.

The Pilots who had to Play it by Ear

An Interview with H. Lee Task

In July 1982, while videotaping the final flight tests of the glide-slope indicator (see *The Pilots who had to be Kept in the Dark - An Interview with H. Lee Task*) from the flight engineers seat of the C-130, Task made an interesting discovery which led to another important project for his group. Task had been lining up his bulky videocamera with a pair of NVGs and the glide-slope indicator on the ground in order to video what the pilot was seeing. Task also had the camera hooked up to pick up the pilot's comments and intra-cockpit communications for the video.

As the pilot came in to land, Task noticed that the navigator and co-pilot were doing a lot of talking to the pilot, providing information about range to the landing strip, altitude, sink rate, and air speed. Task suddenly realized that the pilot was completely isolated from any displays in the cockpit while he was "heads-up" looking towards the landing strip. The pilot was relying on critical information from his teammates in order to avoid crashing into the ground or running out of air speed. He had to integrate the running stream of numbers to maintain some sort of situation awareness.

When Task returned to Wright-Patt, he pointed out this observation to Charlie Bates, the head of the division, and to Tom Furness (a branch chief in the division) during the flight test debrief. As soon as he realized that the crew was reading the pilot's instruments out loud, Task recognized that this critical information could be provided directly into the NVG device. The big question was whether the symbology would distract the pilot from the outside view, or even obscure the outside view.

Bates and Furness left Task to come up with a workable solution. Task drew up a sketch of his envisioned solution in August 1982. He then went down to his machinist and told him what he wanted in order to provide a proof of concept. The machinist put together a housing for an optical system that attached to the front of the NVG lens. A transparent representation of a HUD symbology photograph was attached to the contraption so that the symbology was reflected in the lens in front of the NVGs but which also allowed light from the outside world to produce the night vision image. The result was that the HUD symbology was superimposed on the NVG image. In theory this provided flight information directly to the pilot.

In January 1983, Task showed this to Bates and Furness. This proof of concept was enough to persuade them to fund Task to set up a dynamic mock-up run from an old Apple computer. This demonstration showed that the imagery from the HUD and the filtering of light by the semi-reflective beam splitter in front of the NVGs would not interfere with clarity of the NVG image. The pilots could see both the outside scene and the displayed symbology.

By April 1983 Task had the mock up slaved to the pilot's head position so that the imagery represented the pilot's frame of reference rather than being referenced to the aircraft's heading. The system was demonstrated to MAC, which liked the idea of the symbology in the NVG, but wasn't sold on the head-slaved aspect.

In the fall of 1983 the NVG HUD was successfully flight tested on a C-141. The system has been fitted to many aircraft in the U.S. arsenal and is now a standard piece of equipment. The pilot is now able to see critical flight information while wearing the vision-restricting NVGs, and does not need to rely on that information being relayed verbally from other members of the flight crew.

Lee Task had obtained numerous patents while working for the Human Engineering Division, but this was one that got away. He had applied for the patent, but the administrator in charge of receiving the application rejected it, claiming that it was not sufficiently different from other devices. Task knew that this was incorrect, but he was too busy with other projects to file a rebuttal and the opportunity slipped away.

Warrick's Principle: The Case of the Compatible Controls

An Interview with Mel Warrick

The room temperature is a bit too high. The thermostat shows that it's set for 78°. Which way should I turn the knob to make it cooler? My vertical velocity indicator is showing that my vertical velocity is a little slow. Which way should I move the control wheel to increase it?

Mel Warrick wanted to know how people move controls in such situations. So he set about studying what people's preferences for control movement are and ended up with a law named after him. Some ideas are so simple you wonder if you were the only one to think of it. And then to get a principle about it named after you, well that's laughable. That's Mel's take on the whole Warrick's Principle idea.

An Ingenious Contraption

It seemed pretty obvious to Mel what should happen if you want a display indicator to move one way and you have a dial to control the direction of motion of the indicator, but common sense isn't always so "common."

In 1946, Mel had been invited by Paul Fitts to join him at the Air Force's Aero Medical Laboratory at Wright-Patterson. Mel's background as a science teacher, amateur electronics buff, air force bombardier, and experience in personnel and training issues intrigued Fitts, who thought Mel would fit well into the engineering psychology research program.

Following a study looking at the readability of dials under "excess" accelerations (2-3 G) using the hand propelled centrifuge, Mel began a study to address the stereotypes for control-display movement relationships. Fitts and Warrick had been wondering about the ways that controls work and the expectancies of the operators of these controls.

Their awareness of these issues had been raised by attempts to understand the differences between British and American stereotypes of control operation in aviation and more generally in everyday devices such as light on/off switches. They had also been asked by an engineering lab on the base to look at the adjustment controls in aircraft which moved the pilot closer to the rudder controls. The engineering lab wanted to know which way to move the control for adjusting the pilot's position closer to or further away from the rudder pedals.

Mel's interest in the control/display compatibility area was as much driven by an interest in the building of the experimental apparatus as it was in the study of the phenomena themselves. This personal interest in electronics and gadgets had started as a young boy and had included the building of a set of wings for his bicycle.
Mel set about building what he calls his "ingenious" contraption. Mel wanted to see his subjects'

> During a test "flight" he had actually succeeded in getting the vehicle off the ground. The key lesson he had learned from that experience was the need for something to maintain stability and control of his vehicle once he was airborne. The learning of this lesson was almost as hard as the ground on which he "landed!"

preferences for moving a control knob to elicit a control response. He built an apparatus which had a control knob located underneath the middle light of a row of five. When one of the lights was on, the subject had to move the knob in order to bring the light back to the middle of the five lights. What the subjects did not know was that whichever way they turned the knob, the light would always come back to the center.

The results of the study indicated that subjects turned the knob so that the knob was moving in the same direction as the expected light movement. So, if the light on the far right was on, the subject would turn the knob counter-clockwise (to the left) to bring the light back to the center.

Not so "Common" Sense

Warrick thought he'd merely pointed out the obvious. However, things were not as simple as they first appeared. He had discovered that if you want to move the lights to the left you move the dial to the left. But what if the control knob is placed above the set of lights, does the same simple rule apply? No, it did not. In this case, if they wanted to move the light to the left, subjects turned the dial to the right. What was happening here? The position of the control relative to the display had changed, and so had the movement relationship. In other words, the bottom of the dial now moved to the left. Mel adapted the device to a number of configurations to further test the problem. In one case the lights were arranged in an arc around the control dial, in another case the knob was located on the side of the box with the lights on the facing side.

> This phenomenon is explained by what is now known as the Warrick principle which asserts that the closest part of the moving element of the control should move in the same direction as the closest part of the moving element of the display, as if there was a mechanical linkage between the two.

Warrick's demonstration was followed up with paper and pencil tests and what he calls operational interviews. For these sessions, systems were set up in operational settings and actual operators would use them and report their preferences.

> Warrick never actually named his discovery. It wasn't until later that this display/control relationship principle was formerly named after him, in fact he didn't even know it had been named as such until fairly recently!! His discovery, along with others from the lab, pointed out the need for designers to observe not only how they designed their displays and controls but also where they put them, and how they made them move relative to one another.

For Further Reading

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DISCUSSION

In reviewing the case studies, a number of themes emerge. One is the contrast between projects undertaken with specific users in mind, versus projects intended to have a broader application. A second theme concerns the way the research projects were identified and pursued from the outset. A third theme is the type of organizational support needed by these projects.

User-centered versus Generic Projects

One way to categorize the projects described in this report is whether they were attempting to support a specific user community or not (this distinction is similar to the distinction recognized in the Armstrong Laboratory as fire fighting versus fire prevention). The projects that were directed at a specific user community focused on a single, clearly defined user, and a single, clearly defined need. Earl Sharp's work for the B-52 community and Phil Kulwicki's activities for the F-15 and F-16 SPO fit into this category. So do Lee Task's efforts for the F-16 SPO regarding windscreens, and for the Military Airlift Command regarding night vision landings. The work of Topmiller and Heckart also seems to belong in this category, since the human factors standards they developed were intended for the Ballistic Missile Division, even though the scientists hoped that the human factors standards would be used by other Air Force weapons development programs as well.

For the projects that had a clear user in mind, sometimes the need was narrow and specific. One example is in the case of the fat cursors where Earl Sharp's research team helped work out a better type of cursor design. Other times the research program was more broadly conceived to help a user community handle a new type of challenge.

Other projects were not aimed at any specific user community. Dave Post's development of miniature color displays does not solve any immediate operational problem. Charlie Clauser's effort to define the moment of inertia of body segments had a vague user (someone interested in working on orientation in zero gravity environments) but certainly no one was waiting with a pressing need for this research to be completed. Further, the eventual uses of these measurements had little to do with locomotion in space. Bob O'Donnell's reaction to the request to "measure me workload" also fits this category. The initial concept of helping designers anticipate workload has still not really materialized; one of the primary uses thus far is to measure the effect of chemical and other stressors on cognitive processes.

The time course of impact is different for the two categories of projects. The projects directed at specific users have an impact that is surprisingly fast. One of Earl Sharp's projects, the case of the fat cursors, took only four months from detection of the need, through redesign, through empirical testing, to recommendations. We suspect that there were many other 'projects' that were even faster, perhaps just a telephone conversation in which advice and guidance were offered to a customer who needed a simple fix. In contrast, the generic projects that were not intended to support specific users were much slower, typically taking several years and showing a slower pace during the initial phases.

The degree of impact is difficult to compare. Some of the projects to help out specific users did not have as great an impact outside of these user communities, although it is difficult to trace the nature of influences. Sharp's work for SAC inspired others to set up comparable simulation facilities for other types of aircraft. Other user-centered projects have uncovered general principles that had a wider impact. Kulwicki's studies of high-G performance are of general interest. Task's work on

windscreen evaluation, and on getting symbology into Night Vision Goggles, also has enjoyed widespread application. The work of Topmiller and Heckart easily transitioned into standards that could be used outside the Ballistic Missile Command. We can see from the cases presented here that user-centered research sometimes has a wider application beyond the user group originally targeted.

The generic projects, without specific users in mind, were intended to have a wider impact from the very beginning. That was the rationale for conducting these projects in the absence of an identified sponsor. Post's work on miniaturized color displays can be used in many different ways. In the final analysis, it is not clear if the Air Force will gain more from these miniaturized color displays or from Earl Sharp's solution to the B-52 cursor problem. The Human Engineering Division has pursued both user-centered and generic projects in an attempt to support immediate operational needs and to anticipate the needs of the future.

Trying to Understand the Rainbow

It is only within the last decade that the physics of rainbows has been satisfactorily understood. Yet scientists have been trying to understand rainbows for at least 300 years. Some questions cannot be answered at a given level of technology. Even though the question seems important, little can be accomplished and the scientific inquiry is largely a waste of time. Only when the right analytical techniques have been developed can we expect to make progress. It is not a mistake to try to understand the rainbow, but it can be a mistake to persist before the necessary tools are in place.

The researchers described in these case studies seem to have a knack for picking problems that are important and have just become solvable. They were not wasting their time. They were able to harness new developments and take advantage of opportunities. When Lee Task heard about the way manufacturers were taking measurements on F-16 windscreens, his immediate reaction was that there was a much more efficient method, using lenses. Lee had just completed his Ph.D. in optical science. Five years earlier, before his Ph.D. experience, he might not have seen the opportunity, and he might not have chosen to work on the project. Phil Kulwicki became interested in high-acceleration cockpits in part because he knew about ways of conducting simulations that would not take tremendous effort. Charlie Clauser's interest in measuring moments of inertia was coupled to his knowledge that the relevant techniques had already been demonstrated with baboons. In Bob O'Donnell's case, the cognitive measures approach only made sense because of recent developments in the field, suggesting a set of tools that could be readily adapted to measure cognitive aspects of workload. Ten years earlier the project would not have made much sense. Dave Post's commitment to miniaturizing color displays was based on his discovery that there was a plausible approach using color subtraction. Otherwise he would have turned to other projects when he ran into funding problems.

All the cases that we chose to focus on illustrate the ability to recognize the solvability of an important problem. Of course, these are a sample of a large number of stories, not all of which may have had such great operational impacts. We have no baseline against which to compare hit rates. It is hypothesized that the skill of these individuals is in the seizing of opportunities to pursue higher probability pay-offs.

Many of these case studies show the importance of problem finding. Lee Task hears the co-pilot and navigator reading out instrument data to the pilot and finds a problem he can solve. Phil Kulwicki learns about high acceleration cockpits and finds a set of problems he can solve. Don Topmiller and Steve Heckart try to use human factors guidelines in evaluating new ballistic missile systems and find a problem. No one brought the problem to these researchers. They discovered the

problems and identified them as worth pursuing because they could anticipate that a solution was feasible. Each case seems accidental. Earl Sharp visits a friend and sees videotapes of B-52 Electronic Warfare Officers at work, wrestling with a poorly laid out work station. Lee Task goes along for a night landing to test his glideslope indicators and listens to the pilot getting talked through the landing. Don Topmiller is horrified to see mechanics handling volatile chemicals without wearing gloves. To put these cases into context, we should remember that the researchers were also exposed to many other situations, and observed many other types of difficulties, and did not elect to pursue those. If we envision the problem identification process as a key, then the two tumblers are: seeing a problem worth solving, and seeing an opportunity to solve it. When these two criteria are met, and the two tumblers are lifted, the research engine can start.

In other cases, the problem was already identified. Users complained about inefficient ways of measuring F-16 windscreens, or cursors that obscured the targets. So one tumbler had already been lifted. The researchers needed to feel confident that the other tumbler could be managed—that there was a plausible strategy, an opportunity to be exploited—before becoming engaged with the project.

Anthropologists distinguish between farming cultures and hunter-gatherer cultures. These cases show researchers who are hunters and gatherers. They are not setting out to build multi-year programs. They are trying to solve problems quickly, using whatever resources they can quickly find. Some of them, like Earl Sharp and Lee Task, use their accent into the machine shops on base to quickly fabricate pieces of equipment. Phil Kulwicki starts out by scavenging software programs he can use to run simulations. Earl Sharp scavenges a B-52 simulator. The equipment is a means to a solution, not an end in itself. The case studies show a frequent theme of finding and adapting methodologies, technologies, and data to serve the problems at hand.

A commonality across the case studies is that the researchers were trying to have an impact, rather than waiting to receive instructions and follow someone else's research agenda. Even when someone brought a problem to the attention of the researchers, they took on the initiative to find a solution. This initiative was particularly striking when a project ran into funding difficulties, and the researcher had to scavenge for funds. The initiative extended to cases where rules had to be bent. As Earl Sharp stated, it is easier to ask forgiveness than to ask permission.

Collaborating Communities

No one can succeed in an environment such as the Human Engineering Division without being part of teams and networks. The nature of the team varied in the different cases. Sometimes, the team was made up of other researchers at the Division. Topmiller and Heckart were part of a team that included Charlie Bates, Dave Greek, Lt Kibler, Ken Kennedy, and Milt Alexander. The members of this team recall that period of research with great affection, as a time of shared enthusiasm and purpose. O'Donnell's team was primarily the three groups of researchers working underneath him. Charlie Clauser's team identity included people in the Division, but was centered around professional colleagues and a network that began in graduate school. His effort to measure moments of inertia was a joint project with colleagues at the Civil Aero Medical Institute in Oklahoma City. Dave Post's project also depended on his network of professional colleagues.

In contrast, Earl Sharp's network centered around the B-52 SPO and the user groups. Earl depended on the people in his branch, but his primary allegiance appeared to be to the users rather than to the Division. Phil Kulwicki's network was centered around the advanced fighter community, primarily the SPOs, and the avionics group.

It is tempting to speculate that user-centered research requires tight links outside the laboratory, and generic projects requires tight links inside the laboratory. But there are counter-examples. Lee Task's projects are as user-centered as any in this report, yet he has not forged the same relationship to any user community as have Sharp and Kulwicki. Lee Task's team was a small group at Armstrong Laboratory that helped him put together the equipment he needed and carry out the demonstrations. Another hypothesis is that engineers are more likely to forge alliances with user communities, whereas physicists and behavioral scientists are more likely to affiliate with their professional communities. There does not seem to be any simple generalization about who will form ties with a user community, and who will maintain primary ties to the laboratory team. Credibility with users seems to emerge independent of domain or educational speciality.

The Role of Management

The role of upper management is conspicuous in these case studies by its absence. The term "benign neglect" comes to mind. The researchers find their own problems, form connections with users, get outside funding for small demonstration efforts, then get larger commitments from the sponsors. One common refrain was appreciation that upper management didn't interfere. This appreciation was sincere. The researchers were not in an adversarial role with management. They seemed happy with the people who ran the Division, and grateful for support. But they were also grateful that management did not require them to spend excessive time and energy justifying their programs and documenting their progress. Those who later moved into managerial positions expressed a reduction in job satisfaction and enthusiasm as they lost contact with technical programs. In general, the researchers were pleased to be treated as professionals, and to be given the latitude to follow their judgment about which problems to address. They felt that the non-intrusive atmosphere in the Division was important for their discovery processes. Further, even when the researchers were bending rules they knew that management trusted their judgment.

Only in two of these cases did the initial problem finding come from upper management at the Human Engineering Division, rather than from the researchers themselves or from the user community. One was the request that Dave Post look into miniaturizing color displays, and the other was the request that Bob O'Donnell find a way to measure workload. The first was successful because Post took on initiative after seeing that there was an opportunity, using color subtraction. The second was successful in a more roundabout way—the cognitive test set is not being used to measure workload in order to help equipment designers, but rather to measure the effects of stress and chemical substances on cognitive performance. Other than these two exceptions, the problem finding was not directed by upper management.

Another function of management was to create a cooperative atmosphere. We commonly heard about a conflict resolution style of management that reduced the level of tension by helping people work out differences themselves and seeking reasonable compromises.

The only disappointment we heard was that a few broad-band researchers were frustrated in attempts to gain greater acceptance for their work within the Air Force. They felt that the Division did not have the linkages needed to go the next step, to gain a better hearing for the efforts. They were also frustrated that the Division did not have a means of overseeing implementation once a methodology left Armstrong Laboratory, to make sure that the application was successful. By not having a means of tracking or ensuring follow-through, maybe the impact of the Division on the Air Force is being limited.

Overall, the success of management at the Human Engineering Division has been to create an environment where people can emerge as project champions, identifying opportunities, creating programs, forging alliances. These individuals cannot be selected or identified in advance, and rather than trying for a top-down direction, the management tries to make it as easy as possible for the high-impact researchers to step forward. For example, the division tries to shoulder as much administrative burden as possible rather than passing it down to the researchers. And while the larger changes in the Department of Defense have required a greater level of project justification, and stronger paper trails, management has tried to keep these within bounds rather than using them as excuses to micromanage the work. Instead, upper management seeks to find linkages between projects and recognize commonalities that can be exploited.

Recommendations

Synthesizing these case studies, we can identify some suggestions to help the Human Engineering Division replicate the successes described in this report.

Expanding the tool kit. One theme in these case studies was the realization that a new type of simulation capability or measurement technique or other technology could be brought to bear on a problem. These tools were discovered at professional meetings, visits to other laboratories, word-of-mouth through networks. By expanding the participation in these networks, it may be possible to strengthen the problem finding process. The recommendation is to support activities that will enable researchers to learn about new techniques.

Expanding the communities. By helping researchers have more of a chance to interact with users, and to observe operational settings, it may be possible to strengthen the problem finding process. The recommendation is to support activities that will help researchers interact with operational communities.

Transitioning technology. It is one thing to state this as a Division goal, and another to make it happen. While official programs may be a partial solution, more likely it will take informal networks at the level of upper management to build the bridges and mechanisms needed to turn research projects into Air Force applications. The recommendation is for management to become more active in working with the leadership in operational communities.

Reducing the contract management burdens. Contract management and administration are a part of the research scene, and there may be little the Division can do to help, but it clearly does affect the process and forces researchers to waste time finding ways to get things done without having to go through procurement to award and manage contracts. The recommendation is for management to continue its efforts to protect researchers from excessive administrative burdens.

Dissemination. The case studies featured too many examples of accidental connections to be considered coincidences. The challenge is to increase the rate of these happy accidents. Professional publications and technical reports may not be read by the user communities and by collateral communities even at WPAFB. Possibly other means, such as informal newsletters or electronic bulletins describing work in progress may be helpful. The recommendation is to develop better strategies for describing the work of the division to user communities.

Emissaries. A number of military personnel spend a short time in the Division, often because of their own interest in human factors, and then move to other positions in the Air Force. Several of the case studies were initiated by these emissaries. If there was a way to stay in touch with these

people, and keep them informed about the work of the Division, it might increase the number of happy accidents. The recommendation is to make better use of military personnel after they complete their assignments in the division.

Internal project funds. Based on this small sample, it appears as if the Division is doing a good job with its internal funding. If internal funds were more readily available, it might discourage the user-centered attitude of trying to find problems that would appeal to outside sponsors. The recommendation is to maintain the current strategy of disbursing research funds.

Strategic vision. The recommendation is to continue the strategy of building on division strengths. The division is not attempting to impose a purely top-down agenda, which might actually interfere with the problem finding activities of the researchers. Instead, the division is taking a proactive stance of taking advantage of its capabilities, including the strengths of its leading researchers, to achieve greater impact.

In summary, the case studies describe a set of successful projects that have benefited the Air Force in a number of different ways. The Armstrong Laboratory's reputation is built on the work of its champions, the researchers who initiate innovative research. The work conducted at the lab is a balance of reactive firefighting and anticipatory fire prevention. The researchers choose to pursue different modes at different times. From all the possible choices of projects that they might pursue, they manage to pick a high proportion of projects which often have an immeasurable impact on the human engineering of Air Force systems.

In large part, the projects succeeded because of the initiative of the researchers themselves, and their ability to find the right problems to work on. The Human Engineering Division has historically been able to create an environment where individual initiative is encouraged. These case studies are testimony to the merits of that approach.